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ORIGINAL ARTICLES.

TYPHO-MALARIAL FEVER.¹

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It happened to me to constitute one of a Board of Medical Officers, convened in Washington, in the summer of 1862, to propose a system of reports for the preparation of the medical history of the War of the Rebellion. In the nomenclature then submitted to the Department for adoption was the term *Typho-malarial Fever*. This name, suggested by Dr. J. J. Woodward, was accepted as describing a group of cases, then supposed to be of frequent occurrence, in which the morbid complexus of typhoid fever was modified by a malarial complication. The name, not the conception, was new. The circumstances of the time favored the reception and permanent acceptance of the new term. The nomenclature adopted by the Board was made official, and a large part of the medical profession of the United States, then in the army, were required to adopt it, and the term, which originally was intended as a convenient designation, came to be regarded as the exponent of a pathological condition.

Dr. Daniel Drake, that great medical genius of the West, fifty years ago, recognized the fact that typhoid fever, as was and is the case with many other diseases, received a certain impression from an existing malarial poisoning. The late Prof. Dickson and other observant clinicians, in all parts of the world within the malaria-breeding zone, have long been familiar with this fact. There can be little doubt, I think, that this view has been greatly exaggerated. The typhoid fever of Louis—typical typhoid—is capable of many modifications, of many variations under climatic and other conditions; but in what degree, soever, the clinical history may vary, the morbid anatomy does not vary. Those who study from books rather than at the bedside, are often quite unconscious of the remarkable variations to which the malady is subject, and hence are too apt to find in these departures from the typical, new forms. Dr. Drake saw typhoid as it marched with advancing civilization—as it developed with increasing population—and he fancied that it possessed new features. Dr. Woodward saw the typhoid of the Chickahominy, of an army of unseasoned men, exposed to unprecedented conditions, and he concluded that it was a new malady compounded of the typhoid and malarial disease. Even Drake saw that, in a limited sense, the typhoid poison is antagonistic of the

malarial. Typhoid is the fever of old, of cultivated countries, and dense populations; malarial of new countries and sparse populations. As new countries are opened up to cultivation and to settlement, the original malarial diseases are replaced by typhoid. For a time, and, indeed, during the whole period of malaria production, the symptomatology of typhoid may be somewhat influenced by the prevailing malarial poison. I repeat, *may be*, for, as I shall presently show, typhoid is a *continued* fever, in a certain narrow and conventional sense, only.

The word *typho-malarial*, was made originally by Dr. J. J. Woodward to express his conception of a composite fever. The opinion as to its character entertained by Dr. Woodward has been expressed in his work on Camp Diseases (*Outlines of the Chief Camp Diseases of the United States Armies*, Philadelphia, 1863). On page 202, he says, referring to the morbid anatomy of Peyer's patches, "certain peculiarities cannot fail to strike the anatomist, which are often sufficiently distinctive to enable him to recognize typho-malarial fever by the post-mortem appearances alone." Again, on page 211, he styles it a "new hybrid"—in other words, a cross between typhoid and malarial fever.

Dr. Woodward admits that this view was not accepted by all the medical officers of the army (p. 210). In a paper read before the Medical Section of the International Medical Congress, in 1876, he could name but two opponents of his views—I quote his own words: "Dr. Roberts Bartholow, formerly an Assistant Surgeon in the Army and now a successful practitioner in Cincinnati;" the other "Dr. Jerome Cochran, of Mobile," formerly "a Surgeon in the Confederate Army." If there had been opinions or observations in contradiction of those expressed by Dr. Woodward, certainly only mine had been given formal utterance. My paper in opposition to the specific claim of Dr. Woodward, appeared in the medical volume issued by the United States Sanitary Commission soon after the close of the war. (*Memoirs of the United States Sanitary Commission*, New York, 1867.) In this paper, which appeared soon after Dr. Woodward's views had been published, I maintained that he was in error in supposing that the morbid anatomy of typhoid presented any peculiar features in consequence of a malarial complication. It was not until ten years afterwards, in 1876, that Dr. Woodward retracted the opinion he had at one time expressed, and then not through the official channel, but in a paper to which I have referred, which he read before the International Congress.

I maintained that as populations increase, typhoid supplants malarial fevers; that during the transition period a mixed fever prevails, a typhoid with a malarial complication, but that the morbid anatomy of typhoid remained always the same, and was not

¹ Read by invitation before the Medical Society of Berks County, and neighboring County Societies, in May last, at the City of Reading.

modified as claimed. Dr. Woodward, finally admitted this in the following language: "During my earlier studies I believed that I had observed certain peculiarities in the character of the ulcers in these cases, by which they might be distinguished from the lesions of simple typhoid. A larger experience . . . has convinced me that this opinion was premature. I renounce it as erroneous. There is really nothing in the lesions of Peyer's glands, in these cases, to distinguish them from ordinary cases of typhoid fever." (*Transactions of the International Medical Congress of Philadelphia*, p. 332.) On the next page to that from which I have just quoted, I find this question, and answer: "Is typho-malarial fever a special type of fever? and I reply, unhesitatingly, that it is not." We may then ask, if it is not a special type of fever, why give it a special designation? During the war a scorbutic taint, crowd-poisoning, overwork, affected the constitutions of men, and hence so far typhoid fever was modified by them. Should we not then have a special form of typhoid and a special name for such specimens of the malady? To such a *reductio ad absurdum* do we come by adopting this view.

In regard to the manner in which a malarial infection may modify typhoid fever, there is one source of erroneous observation, in an especial degree responsible for the prevailing misconception. That is—the thermal line of typhoid. Having the notion of a continued fever fixed in the mind, practitioners are too apt to forget that there are distinct remissions and exacerbations of the body heat in every case. There are diurnal variations corresponding to, but in greater relative proportion than the normal. In other words, typhoid fever is, properly speaking, a remittent fever. The likeness to the malarial remittent is all the greater, since in every fully developed case of typhoid, sweating is a pronounced symptom. The differentiation is the more difficult during the first week, since in some cases of typhoid there is constipation, and in some cases of remittent, diarrhoea. In the former, gurgling in the right iliac fossa may be wanting; in the latter, it may be present. A therapeutical means of differentiating, much relied on, may be misleading—I refer to the use of antipyretic doses of quinine—for a distinct reduction of temperature will be effected in both instances by the exhibition of massive quantities. I do not mean to infer, however, that the subsequent influence of the antipyretic will be the same in both diseases.

I fear that the misconception thus originating has vitiated the returns made to the Surgeon-General to such an extent as to render any conclusions drawn from them very misleading. This is the more to be regretted since the volumes containing the medical history of the War of the Rebellion, will have the authority of the Government.

Dr. Woodward, the author of the term typho-malarial fever, having abandoned the original claim as to its solidarity, it may be well now to try to indicate the real relation of the two elements composing the mixed fever.

First, as to the term typho-malarial. This is an unfortunate designation, for it implies the existence

of a malarial fever into which the typhoid enters as an essential element. There is a total absence of proof that such a condition exists. Who has seen the morbid anatomy of malarial fever into which had been projected the changes peculiar to typhoid? It has already been shown that the changes characteristic of typhoid are in no respect modified by malarial poisoning. If then the typhoid poison or germ is received into an organism infected by the malarial poison, it undergoes development in accordance with its own laws. So completely does it dominate the situation that the action of the malarial poison ceases. The term *typho-malarial* is, therefore, a complete misnomer, a misleading phrase, which should be abolished from our nosology; and should no longer appear in our text-books.

Second, as to the supposed hybrid. I much doubt the existence of a typhoid fever, whose symptoms are modified by a malarial fever. Indeed, there are reasons for believing, that in a certain sense an antagonism exists between the two, so far, that in the presence of the typhoid poison, the malarial ceases to be active. I have already given a reason for the belief that the remittent character of typhoid has been a cause of error. At the beginning of the fever, and also during the last week, the remissions and exacerbations are so well marked as to be very confusing. I have always held that the thermal line of typhoid might receive an impression from a co-existing malarial complication,—might, therefore, become more distinctly remittent, but further experience has weakened this belief. If the morbid anatomy of typhoid is not thus affected, why the symptomatology? Examined anew in the light of a wider experience, I have been conducted to the conclusion that the modification in the thermal line, supposed to be due to a malarial complication, has, for the most part, no real existence.

The action of quinine in typhoid fever, and in pseudo-typho-malarial. A further reason for believing in the entire autonomy and independence of typhoid is the failure of quinine to lessen the duration of the supposed hybrid, or to effect more than a transient reduction of temperature. It does not appear, indeed, that quinine acts in any respect differently on typho-malarial fever, than on typhoid. A massive dose effects a reduction of temperature, and in so far as lessened body heat mitigates the severity of the fever, quinine exerts a beneficial action; but that it cannot arrest the disease, or shorten its duration, is absolutely certain. Over and over again has the attempt been made, to cut short typho-malarial fever by heroic doses of quinine; over and over again has failure terminated these efforts.

I must here again point out how an error of diagnosis so often confuses the judgment of practitioners. I have already indicated how many cases of typhoid bear a superficial resemblance to remittent fever. The converse error is even more misleading. The more intense the malarial poison, the more nearly the resulting fever approaches the continued type. The order of form is well known; tertian, quotidian; remittent, of tertian, of quotidian type. If the poison be sufficiently intense, the quotidian remittent presents a thermal line closely approxi-

mating typhoid. Who has not heard of a remittent fever assuming the typhoid state?

I need not say to those here present that a "typhoid state" is far from the typhoid fever. If the nature of a remittent fever with quotidian remissions and exacerbations, is not understood, and sufficiently energetic doses of quinine administered to break it up, the typhoid state is presently assumed. This misunderstanding may readily occur if the history of the case is unknown and its development not watched from the beginning.

When I reported for duty at Fort Leavenworth, Kansas, in 1857, I saw for the first time those deadly malarial fevers which occur in the Missouri River bottom-lands. If such cases had fallen into my inexperienced hands, the most of them would have passed into the typhoid state. Fortunately for the patients and for my future usefulness, the Surgeon in charge of that great military hospital was Dr. John M. Cuyler, of the Army, a physician of great experience, familiar with the practice, then but little known, of massive doses of quinine. To my astonishment, his practice consisted in the administration of a single dose of thirty grains on the first day, twenty grains on the second, and fifteen grains on the third, by which time the fever had practically ended. He indulged in no preliminary skirmishing, no tentative experiments, but well-directed blows with doses that went to their object with unerring precision, were delivered.

Until recently I had supposed that these large doses of quinine, in the treatment of malarial fevers, had been introduced by our army surgeons, as claimed in Coolidge's Army Medical Reports of 1854. In Fenner's *Southern Hospital Reports*, there are several references to this practice in the Southern States. I owe to my friend Prof. W. T. Howard, M.D., of Baltimore, a reference to this practice of still earlier date. In the *Traité Anatomico-Pathologique des Fièvres Intermittent, Simple et Pernicieuses*, Paris, 1825, doses so large as forty grains are recommended in malarial diseases. For sixty years, therefore, it has been known that the severe forms of malarial fever tend to assume a low or typhoid state, unless energetically handled by heroic doses of quinine.

There still remains an answer to the question, What is the nature of the influence exerted by the malarial poison on typhoid fever?

For the reasons above given, it appears to be probable that when the typhoid germ begins its development in the body, the phenomena caused by malarial infection, if it exist, subside. I have held that the only modifications in the symptoms which occur, are the changes in the thermal line, the more pronounced exacerbations, and remissions, the greater excursions of the temperature. I am by no means sure that this opinion is correct. The source of error is so obvious, and mistake is so easily made, that it can hardly be doubted it often occurs.

If the malarial poison remains in abeyance during the predominance of the typhoid action, there comes a time when it asserts itself. After the typhoid infection has spent its force, there occurs an intermittent, which may greatly prolong the convalescence if

not recognized and effectively treated. In my experience, this intermittent succeeds to the typhoid in all cases, in which a really active condition of the malarial poison exists. In the absence of such manifestation, we may well doubt that a malarial complication enters into the morbid complexus. Where the patient is simply affected by that which is styled "chronic malarial poisoning," the influence is insufficient to modify the typhoid process to an appreciable extent. If, however, the malarial infection is active, during the sway of the typhoid fever, it is either overpowered, or, at most, merely increases the daily thermometric range; but as the typhoid process subsides, then the malarial comes into play, and we have a convalescence interrupted or protracted by an intermittent or remittent fever. Often, indeed, have I witnessed these phenomena. I may now close my rather hasty observations, with a sentence from Woodward's paper read before the International Medical Congress at Philadelphia:

"And this brings me at length to answer the question—Is typho-malarial fever, a special type of fever?—and, I reply, unhesitatingly, that it is not." P. 333.

ON THE MALIGNANT METAMORPHOSIS OF DISEASED TISSUES.¹

BY THEO. A. MCGRAW, M.D.,
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THERE are inherent difficulties which make it impossible for even the most experienced surgeon to attain to a uniform success in the diagnosis of tumors. Swellings of the most diverse character frequently present exactly similar symptoms. Normal enlargements even may simulate those which are morbid, and it often happens that the practitioner has to wait for time to develop those differences which alone can make a diagnosis possible. These difficulties arise in a large measure, it is true, from the general likeness of all immature tissues, but there are, besides, others which spring from the tendency of one form of disease to develop into another, or, at any rate, to afford the stimulus which calls that other into being. Thus it has long been noticed that inflammatory and syphilitic deposits precede and perhaps cause the development of cancers and sarcomas, while benign growths not infrequently become malignant. The general practitioner, who necessarily deals but little with this class of diseases, in his anxiety to form a definite diagnosis is apt to forget that nature's boundaries are rarely definite, and that her outlines are often hazy. I propose in this paper to call attention to the practical bearing which these transitional forms of disease have in causing errors in diagnosis. My last winter's practice has indeed afforded an abundant supply of material with which to illustrate this phase of the subject.

Last summer, as I was about to leave Detroit on my vacation, a man thirty-three years of age consulted me about an indurated ulcer of the right side of the tongue, as large as a silver half dollar and in-

¹ Read before the Michigan State Medical Society, at its Annual Meeting, 1884.

volving the whole thickness of the organ. He had a syphilitic history of seven years' standing, and had had the affection of the tongue for one year. A competent surgeon at his home had, as I think correctly, diagnosed the disease as syphilitic, and treated him accordingly. I could just feel on the right side of the neck over the carotid artery, a little swollen gland. I deferred all operative treatment until my return and put him on daily doses of iodide of potassium of forty grains, and had him apply iodide of potassium ointment to his neck.

On my return, three weeks afterwards, I found the tongue in the same condition, but the cervical glands had greatly increased in size. I operated soon after by splitting his tongue in half and removing the diseased portion, and then by dissecting the enlarged glands carefully away from the carotid and jugular sheaths, to which they had become attached. The patient rallied rapidly from the operation, and his tongue was soon well. Not so, however, his neck, for from it there grew a most malignant fungous mass, which in three months ended his life.

Now, here was a case, as it seemed to me, of the undoubted transformation of a syphilitic deposit into a cancerous tumor, and a good surgeon had failed to grasp the reality of the case from an unwillingness to credit a malignant tendency to a specific disease.

A similar case, which I treated for a short time over a year ago, occurred in the person of a married lady who came to my office with a most ugly looking ulcer of the tongue. After a cautious inquiry into the history of the case I concluded it was syphilitic, and put her upon large doses of iodide of potassium. I had in mind a system of treatment which, beginning with a strict antisiphilitic course of medicines, should be followed subsequently, if this failed, by thorough excision.

The ulcer indeed looked to me to have already become cancerous. She came, however, but once or twice, and when I heard of her again she was under treatment by Prof. McLean.

She had in the meantime gone to the Hot Springs, Arkansas, and there had been put on iodide of potassium at the rate of seventy-five grains a day. A temporary improvement followed this treatment, but it was only for a short time. Dr. McLean tells me that when she applied to him the case had become hopeless. The ulcer had eaten into the floor of the mouth, the surrounding tissues had become all molten together, and the neck was full of swollen glands.

She ultimately died of hemorrhage from the lingual artery which had become involved in the general destruction of tissue. In this case, also, I believe that an early and thorough excision would have saved a human life.

Soon after this experience, a poor fellow entered my office with a most undoubted sarcomatous growth of the right upper jaw. He was not yet thirty years of age and had had syphilis, but there could be no doubt that this was something more than syphilis. The jaw was greatly enlarged, the right nostril was closed with a fungous mass, the post-nasal spaces were blocked up with the growth, and the roof of the mouth depressed. It had progressed, in fact, beyond all possibility of an operation, and yet his physician

from the wilds of the northern part of the lower peninsula, wrote to say that I must be mistaken in my diagnosis, as the man was undoubtedly syphilitic.

Whether the previous syphilitic affection in this case had any causal connection with the tumor I cannot positively say, but this seems to me certain, viz., that malignant growths frequently spring from specific degenerations. Practical surgeons have to bear this fact in mind before shutting out a diagnosis of cancer on account of a previous syphilitic history.

I have had three cases of the transformation of tumors, apparently innocent, into highly malignant neoplasms. Two of these cases were moles which had been irritated by the application of caustics. Every surgeon is aware that moles are prone to malignant degeneration, and yet the actual occurrence of the change is sufficiently rare to make such cases worthy of record.

The first case was that of a fine, young man, not yet twenty-two years of age, who was sent to me by my friend, Dr. Small, of Saginaw. Some three months before a congenital mole had been irritated by his collar, and afterwards touched with caustic by a local surgeon. The resulting sore never healed, but became the seat of a rapidly growing melanotic fungus. This was finally excised, but when I saw him one month afterwards, it had recurred not only as a local growth, but also as a metastatic glandular affection. As a forlorn hope, I operated and removed as thoroughly as possible all apparent disease. The operation left a raw space as large as the palm of the hand in the back of the neck and a deep excavation in the subclavian triangle. The patient recovered without any difficulty from the operation, and in a few days was up and about. The operation was performed on Nov. 7, and so rapid was the recurrence that on Dec. 14, the patient, who had left the hospital, except for this cancer in good and robust general health, succumbed to the infection.

The second case was in the person of a man, twenty-six years old, whose home was in Inlay City. He was admitted to St. Mary's Hospital on January 3. Two years before a mole on the upper part of the right forearm had been irritated and finally destroyed with caustic. A year and a half afterwards, a tumor was observed on the lower part of his right arm, which was imperfectly extirpated by a local surgeon, after it had grown to the size of a hen's egg.

The wound never healed, and became the seat of a rapidly growing secondary tumor.

Some infected glands could be distinguished in the axilla, which in three days grew very perceptibly in size.

On Jan. 7, I amputated the right arm at the shoulder-joint, and removed a large mass of glands from the axilla.

This patient recovered and went home, but on May 20 returned with a new lump in the axilla, which I removed on the following day. He is now recovering from this latter operation, and anxiously debating whether he has yet escaped his enemy.

The first of the tumors proved to be a round-celled sarcoma. The second, a spindle-celled sarcoma. They may serve as a warning to such practitioners who recommend the destruction of moles

by mild caustics. I would not myself use any caustic whatever for a mole, preferring the safer method of excision, but he who uses a caustic for such purpose should choose one of the strongest, such as could destroy at one application not only the tumor, but also some of the underlying tissues. I am firmly convinced that not only in these cases, but also in cases of epitheliomata of the face, mild caustics, which reach only the superficial layer of cells, often do frightful damage. The lower strata, stimulated into activity, grow into the depths more rapidly than the caustic can penetrate, and a tumor, whose power for evil may have been but latent, suddenly develops a dreadful malignity. I do not mean, of course, to say that every irritated mole turns into a cancer, but who would take any chances when such possibilities lie before him?

Another patient, in whom I have this year had the sorrow to witness the transformation of apparently simple growths into highly malignant tumors, was a woman of some forty-one or forty-two years of age, whom I examined carefully two years ago for enlargement of both breasts. She had then noticed for several months growing tumors, which had attained, when I first saw her, to the size, in the right breast, of a hen's egg, in the left, of a large walnut. They seemed to be cystic, and, when I punctured them, I was able to collect about two ounces of a reddish-brown fluid. The microscope revealed in the fluid a few shrunken cells, some blood corpuscles, more or less altered, and granular matter. After the puncture, the walls of these cysts collapsed so completely that it was then difficult to find any abnormal swelling whatever. There were no perceptible indurations or nodosities. I came to the conclusion that the trouble was cystic and innocent, but requested the patient to call from time to time for examination. I saw no more of her for two years, until about six weeks ago she came to me again with both breasts the seat of extensive and unmistakable scirrhus cancer. The disease had reached an incurable stage, and my too favorable opinion had caused the loss of that golden time when a cure might have been possible. I have my own theory about those cysts of the breasts which occur in the persons of middle-aged women. They occupy dilated ducts, and may be classed with retention-cysts. Now, according to Cohnheim, there are two elements essential to malignancy. The first is the aggressive growth of certain groups of cells. The second is the loss of resisting power in the neighboring tissues. Cancer of the breast begins in the acini or the smallest lacteal ducts by the riotous proliferation of epithelium, but becomes cancer only when this epithelium has succeeded in forcing its way into the connective tissues immediately adjacent. If now this growth takes place, but the connective tissues sturdily resist its encroachment, the cell will eventually die and accumulate in the distended ducts. Their presence causes irritation, and the secretion of more or less fluid. Cysts form then which contain only such fluid and the debris of cells, and are apparently simple. At a later stage, however, the connective tissue becomes more atrophied with advancing age, and yields to the epithelial pressure. We have then the

development of malignancy in growths which we had fondly believed innocent. I have been long suspicious of certain of these so-called retention-cysts, but have never until now had so marked an example of their dangerous tendency.

Of all complications, which are apt to mislead the surgeon in his judgment of abnormal swellings, the advent of tumor in inflamed and injured parts is the most deceptive.

By one of those curious coincidences which occur so often in practice, one physician was able to send me, almost together, two cases of sarcoma of the femur, in both of which a history of hurt and subsequent inflammation had deceived a most competent practitioner as to the true nature of the case.

Charles Coots, aged 24, had been in good health until last September. He was employed in a planing mill, and, while at work, sat at a table on which a wheel was turned by means of a leather band, which passed into the room below through an opening in the floor.

The constant draught of air through this opening, and the occasional rubbing of this band, caused an intermittent pain and soreness in his left knee. This became so severe that he was finally compelled to quit work and keep the house. He was treated for inflammation of the knee-joint for several months, and the fluctuations between apparent improvement and exacerbation of trouble seemed to establish the correctness of this diagnosis. On Jan. 10, 1884, he fell, and afterwards grew rapidly worse. It became evident that the case was one other than that of simple inflammation, and I was sent for in counsel. I advised amputation, and performed it at St. Mary's Hospital on Feb. 27, before the students of the Detroit Medical College. The patient was much reduced at this time, and I amputated between the middle and upper thirds of the thigh. I had reason soon to regret that I had not operated at the hip-joint, for a close examination of the stump showed numerous black points in the medulla of the bone, due to the presence of pigmented cells, although the soft parts seemed perfectly healthy. This demonstration of the greater speed with which the infection of tumors travels in the medullary cavity of bone than in the surrounding flesh was very instructive, as showing the folly of amputating in such cases through the bone. I believe that the rule ought to be universal, where the bone is involved, to disarticulate at the joint above. The patient was too weak for any further operative procedures, and he was left to recover from the operation with the certainty that subsequent amputation would become in time a necessity. The femur in this case was found to be the seat of an enormous melanotic sarcoma, which had involved the knee-joint, and almost entirely destroyed the lower part of the bone.

The second case of this kind occurred in the person of an old soldier, W. E. Lerrick, who had received a shell wound in the left knee during the late war, and had ever since suffered from lameness and stiffness of the joint. As the result, apparently, of a slight injury received about a year ago, the knee began to pain him again, and get hot and swollen. The diagnosis of inflammation was naturally made, and for

many months he was treated with various antiphlogistic measures. He was finally sent to St. Mary's Hospital, where he arrived on March 14, 1884. He had then an enormous tumor involving nearly the whole thigh, and ulcerated at the knee. No enlargement of the lymphatics could be detected. As a last resort, on March 17, I amputated at the hip-joint. The tumor proved to be a white round-celled sarcoma, and I was able to show by sections of the bone, as the trochanters, that the whole bone was involved in the disease. The patient is nearly recovered, and left for his home on June 10, a small sinus still leading into the cavity of the acetabulum. Now, in these two cases, I think that the most experienced surgeon would have been liable to mistake the nature of the trouble in the beginning, and every practitioner knows how difficult it is to escape from a prejudice when once it is formed. It has been said that any inflammation may prove infective, and I have had very many cases in my own practice in which cancer has seemed to graft itself upon inflammatory tissue. Whether the two which I have related were of this kind, or whether they were from the beginning cases of mistaken diagnosis, it would indeed be hard to decide.

I have just performed a palliative operation upon a woman for a large cauliflower growth of the uterus, which was discovered six months after childbirth. These cases are so common as to fall within the observation of every gynecologist of large practice, and are attributed by many to cervical rupture, the raw surfaces of which become irritated and covered with granulations, the cellular elements of which, instead of undergoing cicatricial contraction, multiply enormously, and acquire a property of infection. Whether this be so, or whether they spring from some minute placental remains or from residual foetal cells, this much is true, that such growths follow close upon processes, which, if not altogether normal, would nevertheless have an entirely different result in by far the large majority of cases. It is by no means the only class of cases in which granulation-tissue is the starting-point of cancer, but it will serve here to good purpose as an example, because such tumors are often overlooked in the early part of their history, their peculiar symptoms being masked by those sequelæ of childbirth which cause sometimes for months morbid discharge from the uterus and vagina.

As regards the diagnosis of these cases, in which morbid tumors are suspected of forming in otherwise diseased tissues, I must confess that I know of no criterion by which we can render ourselves sure of our diagnosis in the early and transitional stages. The microscopic examination of a few detached cells gives but an uncertain and doubtful result. The examination of any large mass of tissue can be done only by an exploratory operation, which to most patients would seem as formidable as the radical operation, and we need to satisfy ourselves of the diagnosis before urging the patient to such severe measures. I have frightened some patients away even by proposing an exploratory puncture with a small needle. The fact is, time is the great solver of many of these diagnostic riddles, and if surgeons only guard themselves against those prejudices which

fix themselves in the mind after a false diagnosis has once been formed, errors in opinion may often be rectified in time to operate efficiently and thoroughly. Ulcers which are persistent in places where ordinary ulcers ought to heal, syphilitic sores which resist the appropriate treatment, cysts which refuse to heal after the evacuation of their contents and destruction of their lining membranes, uteri which continue to discharge blood and debris an undue time after delivery, all ought to awaken suspicion, and incite the practitioner to the most thorough examination. If then the cause remains doubtful, the patient ought to be given the benefit of the doubt, and those severe and radical measures used which would be applied in cases of pronounced cancer.

DIRECT APPLICATION OF THE GALVANIC CURRENT IN SALPINGITIS.

By HORATIO R. BIGELOW, M.D.,
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I WISH to call attention to what may be considered a unique case in gynecological therapeutics. It is not sufficient of itself to establish any definite principle, although it may lead up to such investigation as shall place this procedure upon a logical basis.

I have had under observation for many months a case of oöphoritis and salpingitis that made but slow progress under the systematic treatment of electricity, massage, Turkish baths, and graded muscular exercise, that I always resort to in these conditions. The usual symptoms were well marked—objective and subjective. The sound passed easily and for quite a distance up the right tube. Menstruation was irregular, painful, scant, and altered in character. It was perhaps a suitable subject for Tait's operation, but we cannot all hope for the brilliant results of that eminent surgeon, and I have already expressed my views upon oöphorectomy, as well as upon the general management of oöphoritis.

I had previously seen excellent results from the galvanic current; one electrode (negative) being pushed well up into the vagina, while the other was applied externally over the location of the ovary. It seemed to me that an insulated electrode of the size of the ordinary uterine sound, might easily be carried into the tube, quite as easily, indeed, as the sound itself, and that the negative current might alter the entire catarrhal condition of the membrane, setting up a new action—a reparative one—within the tube itself, and that this influence might be transmitted well up to the ovary. At all events, it was an experiment worth trying. I knew from an extended use of electricity in the diseases of women, that both currents were well tolerated by the sexual apparatus; indeed, the uterus is only slightly susceptible, less so than any other part of the body.

I had made an electrode of the same dimensions as my uterine sound—insulated—and terminating in a bulbous metal expansion. This was carried without the least difficulty well up into the tube and was made negative. The positive electrode was

pressed over the external parietes in the location of the ovary. I began with a current strength of five milliamperes, continuing the application for three minutes. The positive electrode was small, and pressed firmly down. The patient made no complaint; on the contrary, she confessed to a feeling of satisfaction at the sense of warmth. I made applications twice a week, varying the strength of the current, but never beyond twelve milliamperes. She always felt better after each séance, and now, after three months of this treatment, is really radically better. She has little or no pain; the menstrual function is accomplished quite naturally, and the enlargement of the tube is constantly growing smaller, so that it is now impossible to admit the sound.

May it not be possible that we have here such a powerful alterative remedy, as will relegate to cystic and well advanced degenerative changes only, the necessity of the surgical operation?

HOSPITAL NOTES.

CINCINNATI HOSPITAL.

Service of N. P. DANDRIDGE, M.D.

(Reported by W. H. WILDER, M.D.)

EXTRACTION OF A BULLET FROM THE URETHRA; TWO CASES OF EXTERNAL PERINEAL URETHROTOMY.

FRANK STARK, colored, groom, aged twenty-six, well developed, was admitted to the Cincinnati Hospital December 17, 1883. Says he contracted syphilis four years ago, and gonorrhœa, for the first time, eighteen months ago—recovering from the latter trouble rapidly. He remembered that on a certain day, about one year previous to his admission, he suddenly found himself unable to micturate, and consulted a physician, who introduced a catheter and also prescribed certain medicines, from which, he says, he derived no relief.

Since that date he has been unable to void his urine in a full stream, but it escapes from him in drops, and any attempt at micturition is accompanied by severe pain. When admitted, the bladder was distended, and the patient complained of some pain and uneasiness.

An unsuccessful attempt to introduce a small rubber catheter having been made, he was ordered a hot hip-bath, gr. x pulv. ipecac. comp., and rest in bed.

The next morning, Dec. 18, he was feeling much better, having passed urine more easily than before. The urine was alkaline, and contained an abundant deposit of phosphates, and very tenacious viscid mucus, with some pus.

Dec. 19.—He said that the obstruction appeared very suddenly, and that the day before he first noticed it he had been able to pass a good-sized stream. On introducing a sound, a hard body was struck, about four and a half inches from the meatus urethrae, which gave the click of a stone when touched by the steel. This foreign body could be felt as an irregularly shaped point of induration, at the peno-scrotal angle.

20th.—Dr. Dandridge attempted to extract the foreign body with forceps, but it was so firmly held that it was necessary to make an incision along the median

raphé at the junction of the penis and scrotum down to the point of a sound which had been introduced into the urethra, and which was pressing upon the point of obstruction. A leaden bullet, about $\frac{1}{16}$ inch calibre, considerably flattened and indented, nearly covered with urinary salts, and firmly embedded in the tissue of the urethra, was removed with some difficulty. After the operation, a No. 16 English sound was easily passed into the bladder; the wound was dusted with pulv. iodoform, and the patient ordered to remain quietly in bed.

In the evening of the same day, the pulse was 96, temperature $100\frac{1}{4}^{\circ}$, and the patient very cheerful. At that time he related an incident which threw light upon the presence of the bullet in the urethra. In 1870, he received a pistol shot in the left groin, and although careful search was made for the ball by several surgeons, it was never found. A small circular cicatrix, about one inch below the middle of Poupart's ligament of the left side, indicated the point of entrance of the missile; and several scars, about an inch in length, on the inner and posterior surfaces of the thigh were evidences of fruitless exploratory incisions for extracting the same.

26th.—Pulse 72, temperature 99° . Complained of some pain in scrotum, which was slightly swollen. A suspensory bandage was ordered, and the iodoform dressing continued. Patient was instructed to micturate in the knee-elbow posture, at the same time raising the scrotum from behind, to prevent as far as possible any gravitation of the urine into the tissues of the scrotum.

31st.—No. 14 English sound was passed into the bladder. Patient was improving rapidly. This sound, No. 14, was passed daily until the date of his departure. When discharged, on January 19, 1884, the wound had nearly closed. Micturition took place entirely through the penis, and the patient was feeling well.

Remarks.—I can find no case on record in which a missile of any kind having entered the bladder, has subsequently become impacted in the urethra. In the *Medical and Surgical History of the War of the Rebellion*, Part II., surg. vol., there is a record of twenty-one cases of vesical calculi consequent upon shot wounds of the bladder. Thirteen of these calculi had developed a round a missile, such as a bullet or fragment of a shell, this acting as a nucleus; and small fragments of bone, cloth, or hair had constituted the nuclei around which the other eight had formed. None of these entered the urethra, probably on account of the large size of the bullets or fragments; and all were removed by lithotomy operations. There are on record five cases of individuals who received gunshot or pistol-shot wounds of the bladder who subsequently passed the bullet through the urethra. Three of these cases are quite ancient, having been cited by Elschoitz, in 1693; Thomas Bartolinus, in 1680; and Mangther, about 1721. The other two were reported, in 1855, by Dr. C. D. Stickney (*Boston Med. and Surg. Journ.*), and, in 1867, by Assistant Surgeon J. V. Lauderdale (surgical volume of the *Medical and Surgical History of the War of the Rebellion*). In these cases, the balls were small enough to escape through the urethra during a forced attempt at micturition. The passage of small fragments of bone by the urethra after gunshot injuries of the pelvis and bladder was much more common in the late war; and in one case, some bone-fragments, which had become impacted in the

deep portion of the urethra, were extracted by Dr. Thomas G. Morton by perineal section.

In the case of Stark, the obstruction had evidently existed for over a year, with severe vesical tenesmus and painful micturition; but how long the foreign body had been impacted in the position where it lay could not be determined.

Its jagged surface had caused it to become firmly embedded in the mucous membrane, and the irregular surfaces left space for the urine to trickle past the obstruction.

After introducing the sound, the thought of the presence of an impacted vesical calculus was entertained, and an attempt was made to crush and remove it with forceps; but, this failing, removal of the foreign body through an incision became necessary.

It seems evident, in this case, that the bladder was not perforated originally by the shot, for no manifestations of vesical trouble appeared prior to a date about one year before his admission to the hospital, but that it subsequently entered by a process of ulcerative absorption, having probably worked its way through the loose connective tissue of the pelvis to a position alongside the bladder-wall.

Had there been made a proper examination when he first complained of vesical symptoms, the discovery of the foreign body would have followed, and a lithotomy operation would have accomplished his relief. Although a linear incision through the corpus cavernosum into the urethra is not likely to cause a constriction of that canal after the wound has healed, yet, in this case, we can reasonably apprehend a form of traumatic stricture, owing to the lacerated condition of the urethra, caused by the pressure of the jagged body.

MEDICAL PROGRESS.

THE TREATMENT OF STRICTURES OF THE OESOPHAGUS.—PROFESSOR J. HJORT, of Christiania, read a paper on this subject at the International Medical Congress at Copenhagen, in which he concluded that:

1. In cases of cicatricial stricture of the oesophagus—even in its upper part—impermeable to instruments, gastrostomy *en deux temps* is indicated; and then to treat the stricture, through the fistula, by means of electrolysis, as has been successfully done in one case.

2. It is also probable that the treatment by electrolysis should replace internal oesophagotomy in cases of cicatricial stricture, still possible for a small sound.

PHOSPHATED PEPTONES IN OBSTINATE VOMITING OF PREGNANCY.—DR. A. JUDET calls attention to the necessity of administering phosphate of lime in pregnancy and during lactation. This is particularly indicated when the father or mother has a scrofulous diathesis. By its action it is an element which meets the drain upon the osseous system of the mother; and being an excitant of nutrition it causes assimilation of the albuminoids, thus fortifying, in a measure, the organism and placing the mother in the best conditions for furnishing without fatigue the proteic elements necessary for the development of the fœtus.

He reports the case of a woman, twenty-seven years of age, who was having attacks of uncontrollable vom-

iting, being in the third month of utero-gestation. Chloral and valerianate of caffeine gave only temporary relief. Phosphated peptone in an infusion of bitter orange-peel was then given, six teaspoonfuls of the former being given during the day. After slight symptoms of intolerance, this was well retained and the proportion was gradually increased to twelve teaspoonfuls a day, this being the only food taken. This exclusive regime was kept up for five days, after which she began to take small quantities of other food. The peptone was continued in doses of two to four teaspoonfuls a day not only during pregnancy, but during the whole time of lactation. Digestion, which after all former pregnancies had been painful, was now performed in a normal manner. In previous pregnancies she had been unable to nurse her children, but she nursed this child during the full period, and its dentition was much earlier than that of any of the others.

It should be remarked that there are differences in phosphate of lime, and it should be carefully selected, and furthermore it is assimilated with ease by the system only when combined with a nutrient proteic substance. Peptone and phosphate of lime make a very rational and *physiological* preparation for building up the osseous and muscular systems.—*Gazette Hebdom.*, August 8, 1884.

TWO CASES OF MYOMOTOMY.—E. BIDDER reports (*St. Petersburg med. Wochenschr.*, 1884, N. F. Jahrg. I., No. 1) two cases in which he has performed this operation. The first case, in which laparotomy was performed by Schröder's method, easily recovered. In the second case, there was a tumor of the posterior cervical wall, which reached over an inch above the symphysis. The body of the uterus, of normal size, was on the anterior portion of the tumor. The tumor was removed *per vaginam*. After slitting the anterior lip of the cervix, a wedge-shaped piece was removed from the tumor, the base of which formed the posterior lip. The vagina was then packed with iodoform-gauze. During the course of the next week, the tumor sank into the vagina, and became partially freed from its capsule, filling the vagina as completely as would a child's head. On the twelfth day, it was removed with the fingers. An hour after the removal, tolerably free hemorrhage set in, which was controlled by a tampon. Recovery took place quickly, and without fever.

The tumor was six inches long, and a little over three inches wide, and about the size of a child's head. From the ease with which this tumor was enucleated, Bidder advises that these tumors be removed through the vagina whenever possible.—*Centralbl. f. Chirurgie*, August 2, 1884.

EXTIRPATION OF THE UTERUS THROUGH THE VAGINA.—DR. E. KUFFERATH, of the University of Brussels, reports, with a general consideration of the subject of extirpation of the uterus, the case of a woman, forty-five years of age, from whom he removed the uterus by the vaginal method. The disease was of two years' duration. The wound was completely cicatrized six weeks after the operation, and the patient seemed perfectly well for four months and a half, when infiltration again appeared.—*Annales de Gynéc.*, July, 1884.

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ARSENIC IN MALIGNANT LYMPHOMA.

THE treatment of malignant lymphoma or lymphadenoma, which should not be confounded with sarcoma of the lymphatic glands, is most disappointing, as the disease almost invariably continues to progress until death ensues from sheer exhaustion. Now and then a cure follows extirpation or the parenchymatous injection of tincture of iodine, hyperosmic acid, or Fowler's solution; but the local and internal administration of arsenic, which was brought prominently into notice by Winiwarter, in 1875, and is so much vaunted by many surgeons, has afforded so little satisfaction that it is rarely resorted to out of Germany.

Attention has again been called to the arsenical treatment of this class of morbid growths by KAREWSKI, in a paper read before the Berliner Medicinische Gesellschaft, and published in the *Berliner klinische Wochenschrift*, Nos. 17 and 18, 1884. Of the four cases, three are recorded as cured, and one as improved, there having been freedom from recurrence for four years in one, and twenty-nine months in two. In the first, the pharynx, and the cervical and axillary glands were affected; in the second, the tonsils and the glands of the neck, axillæ, and groins were the seats of the disease; while, in the third, in addition to the tumors designated in the second case, the liver and spleen were greatly enlarged, and the patient was suffering from diffuse bronchial catarrh. In the first and third cases, the remedy was employed hypodermatically and by the stomach, and in the second, by the stomach alone. After a few injections the tumors began to diminish in size,

and the diminution went on so rapidly that in from six to eight weeks glands of the volume of a hen's egg were reduced to that of a cherry. Not only was this good result witnessed in the tumors injected, but also in distant growths, the tonsils resisting the influence of the remedy longer than the other glands.

In the discussion which followed the reading of the paper, ten additional cases were reported by Güterbock, Küster, Heymann, and Grunmach, which tend to demonstrate that the prognosis of malignant lymphoma, when subjected to the arsenical method of treatment, is not so bad as it is usually regarded. There can be no doubt that in the fourteen cases life was prolonged, but whether a final cure resulted is quite another question. In three of the fourteen there was recurrence of the disease, and the reports would have been far more conclusive had the time that had elapsed after the so-called cure been recorded. As we have seen, the patients of Karewski remained well, with a *persistent slight enlargement of the affected glands*, for three years, on an average; but, as examples of recurrence after two years and more have been recorded by Winiwarter, Zesas, and others, not a single one of the cases here noticed can be said to be safe from recurrence.

Our own experience with the disease indicates that there is no tumor which recurs so surely after extirpation, or the arsenical treatment, or which is so surely fatal, as the one under consideration. Hence, we are disposed to accept these cases not as cures, but merely as prolongations of life. From the influence exerted by arsenic upon the primary and recurrent growths, we are of the opinion that malignant lymphomata should, when practicable, be excised, and recurrence be guarded against by the administration of Fowler's solution by the mouth.

THE ETIOLOGY OF INFLUENZA.

NOTHING in medicine has more completely set at naught the vaunted investigating power of man than the cause of influenza. Its very name, now adopted by common consent out of a host equally vain, originally denoted its supposed mysterious nature, and the uselessness of scientific effort to find out by searching its cause. The elders turned to the elements, earth, air, fire, and water, without result. They were, perforce, content with the theory of some occult "influence" from the stars. In later days, when the nature of influenza and the manner of its epidemic spread came to be well known, the mystery of its cause still remained wholly unsolved. Even recent epidemics, studied under the most favorable conditions for research, have shed no light upon its actual cause.

It has been easy to show, from time to time, what it is not, and to overturn high castles of theory that

stood upon no base of fact. But the spoiling of the grapes went on, and the little foxes were not found. Then opinion came to drift miasmward. What is miasm? In this instance it was something that accumulated in the northeast, beyond Tartary, and was puffed by Boreas, when there was enough of it, across China, Russia, all Europe, and the high seas to the New World, and sometimes around again to far Cathay. Certainly, these vast pandemics were a puzzle. Whatever the cause of them, it could only be transmitted by the most general of media—the atmosphere. Its devastations were like those of the locust, but the field upon which it feasted was the wide, wide world. Surely it bred as it journeyed; else it would have been dispersed to the four winds of heaven in traversing areas neither bounded by continents nor limited by seas. Moreover, colonies of the miasm found abiding places and were left behind; and thus local outbreaks took place from time to time, after the great pandemics were ended. Hence, men came to think and speak of this miasm of influenza as something organized—living, and to explain it by the germ theory of disease.

Happier in his suggestion of the truth than in his versification was the poet who wrote:

"So, naturalists observe, a flea
Has smaller fleas that on him prey;
And these have smaller still to bite 'em,
And so proceed *ad infinitum*."

And this particular flea, like the celebrated tormentor of the Irishman, eluded capture. When you put your finger on him, he wasn't there. There has now, however, arisen in Würzburg an investigator who has succeeded where all who came before had failed. Otto Seifert is his name. With his sling and a pebble from the brook he has brought down the giant whom the rest went out against in vain. And the manner of it was as follows: Dr. Otto Seifert had the opportunity of observing an epidemic of influenza in Würzburg. It was not exactly a pandemic. The first case came under his notice, in the Julius Hospital, on the 14th of January, 1883, and the last on the 24th of March, in the same year; and they numbered, in all, *seven*. He was, therefore, enabled to cope single-handed with the whole epidemic. He not only routed it, horse and foot, but he also took it captive, and has studied its appearance and habits with the care and interest due to so distinguished a prisoner of war. As the reader anticipates, it is a coccus, and a micrococcus at that. Enlarged 350 times, its length is from 1.5μ to 2μ , and its breadth 1μ ; and its number in the sputum is legion. It arranges itself mostly in long chains, the rank and file, less often in pairs (diplococci), and occasionally it stands forth alone (monococcus). It was caught in the secretions of the nares, and in the sputum, in

the earlier actions of the campaign. Later, when these became fouled and purulent, the coccus forsook them. In the tears, even when they most bedimmed the eyes, it was not found. This captive was clad by its conqueror in a robe of methyl-violet, and made a magnificent appearance.

In order to establish its identity, laborious comparisons, of the nature of control-experiments, were made. Examinations of the same secretions of many other sick than the seven were conducted, but with a negative result. The coccus was not there; nor was it found in any blood—even of the seven. The inoculation of two white guinea-pigs failed. Like other fierce captives, it grew sullen, and refused to do its trick. Or was it like those noble beasts which become sterile in captivity? Dr. Otto Seifert magnanimously gave it a new robe, of almost Tyrian purple, but the spirit of the victor prevailed, and he has called it by a name not its own—"Influenzococcus," and has broken its spirit by likening it to the base coccus of gonorrhœa.

A full, true, and particular history of this discovery will be found in Volkmann's *Klinische Vorträge*, No. 240, 1884.

ACUTE INFECTIVE OSTEOMYELITIS.

In the *Fortschritte der Medicin*, Nos. 7 and 8, 1884, may be found an interesting paper on the part played by micrococci in acute osteomyelitis, from the pen of KRAUSE, of Halle, whose researches confirm those of Struck, made in the laboratory of the Imperial Board of Health of Berlin, under the direction of Koch. The pus with which the investigations were made, and which was obtained from nine cases of osteomyelitis in man, disclosed, after careful culture, the usual orange-colored micrococci, and, in addition to these organisms, in one case, a white coccus, which had previously been described by Rosenbach as occurring in three examples of closed osteomyelitic abscesses, and to which he gave the name *staphylococcus pyogenicus albus*.

In the experiments made upon eighteen dogs and seven guinea-pigs, with a view to reproduce the disease by inoculation, the culture-fluid was mixed with two parts of water. Small quantities, whether introduced under the skin or into the peritoneal cavity, were followed by negative results, and the intraperitoneal injection of a larger quantity merely set up peritonitis, without any indications of lesions of the bones. When, however, the bones were broken, an abscess frequently formed at the point of fracture. Thus, of fifteen cases, suppuration ensued in seven after a single injection, while in three others several injections produced a local abscess in the vicinity of the fracture, as well as in the neck of the femur in one. In three, in addition to the abscess at the site of the fracture, there was extensive suppuration of the me-

dulla. In all, the pus, as well as the cultures, contained the characteristic micrococci.

The intravenous injection of putrid matter was resorted to in four dogs, in which one of the bones of the extremities had been broken. Abscesses formed in every case at the seat of the injury, but without extensive suppuration of the medulla; and the organisms present in the pus were entirely different from those found in the pus of osteomyelitic abscess.

The results of his experiments lead Krause to the belief that the lesion produced at the site of a fracture in the lower animals by the inoculation of the cocci of osteomyelitis is not the same as osteomyelitis of man; and that the cocci of acute infective osteomyelitis of the human subject are merely possessed of extraordinary pathogenic and pyogenic properties.

A SUPERANNUATED OPINION.

BUDIN says the opinion that the foetus is nourished by the amniotic liquor is singular and superannuated, and is unworthy of discussion. Charpentier states that the theory which regards this fluid as nutritive, and that the foetus swallows and digests it, has now sunk into an oblivion from which it can never escape. But, how little prophets know, for the theory has escaped from oblivion and appears in *The American Journal of Obstetrics* as a new-born child, not as an exhumed corpse! Among the arguments adduced in its support we fail to find the one which Hippocrates used, to prove that the child sucked while in the womb, for, said he, if it did not suck then, how would it know how to suck when it was born? so, if the child did not swallow liquor amnii in the uterus, how could it swallow milk at birth?

Certain proved facts militate against the new-old theory, and a few may be mentioned. The size of the child is usually in inverse proportion to the quantity of amniotic liquor; according to the theory, the more of the liquor the greater ought to be the size of the child. It is hardly probable that a fluid which often, if not always, contains products of retrograde metamorphosis is designed for food. Acephalous monsters are often well developed, but they have not been able to drink a drop of this asserted sole nutritive material for the foetus. Experiments upon the young of inferior animals have shown that a diet exclusively of amniotic liquor means starvation.

It has been shown by WIENER in a recent number of the *Archiv für Gynäkologie*, that in the latter part of foetal life the function of many organs can be exercised just as after birth, and especially that the great development of the follicular apparatus of the mucous membrane of the stomach and intestines enables them to perform digestion, but as these organs usually contain excrementitious matter, their activity

is not important. It does not seem to us that an argument for the exclusive nourishment of the foetus by the swallowing and digestion of liquor amnii can rest on the condition of the digestive apparatus, any more than we can assert that the special senses of sight and hearing are exercised before birth, because the organs of these functions are sufficiently developed for action.

PSEUDARTHROSIS OF THE TIBIA.

AN ingenious method to restore the usefulness of the limb in pseudarthrosis of the tibia, following great loss of substance in complicated fractures, is described by HAHN in the *Centralblatt für Chirurgie*, May 24, 1884. The conical extremity of the upper end of the tibia having been removed, the fibula is divided at the same level, its lower extremity implanted in the medullary canal of the tibia, and the lower end of the tibia refreshed in an oblique direction, in order to strengthen laterally the support afforded by the fibula. The plan was successfully carried out by its deviser in a case in which the defect between the extremities of the tibia measured three inches and two-fifths.

POLLUTION OF THE SCHUYLKILL RIVER.

THE efforts which have been made to protect the Schuylkill River from pollution from sources within the limits of Philadelphia have been only in part successful. By the acquisition of Fairmount Park many nuisances on both banks of the river, which contributed toward the contamination of the water, have been abated. But there still remain numerous and well-known sources of sewage contamination of the most repulsive and dangerous character, which, at certain seasons of the year, and in certain conditions of the stream, have caused the water to be exceedingly unwholesome. Notwithstanding these facts, and in the face of the protest of citizens, every effort made to have the evil removed has been a failure.

Several years ago when legal proceedings had been commenced against certain mill-owners for discharging deleterious substances into the river, City Councils ordered the suits to be discontinued, for the reason, as surmised, that interference with industrial establishments would have the effect of driving them beyond the city limits. Whatever may have been the motive which induced this unwise action, the result has been to perpetuate a nuisance which has been increasing in magnitude year by year until at last it has become imperative that the rights and health of the people should be protected.

Upon evidence collected by the Chief Engineer of the Water Department, the City Solicitor has been directed to prosecute the offending parties, and, with

the support of the Board of Health, this officer has commenced proceedings against a number of persons who have been bound over to answer at court for creating and maintaining a nuisance injurious to the public health.

These proceedings will be watched by the community with the most intense interest. The hope is that they may be successful for their direct results, and also in establishing a precedent which shall serve as a guide in similar cases. If not successful, the burden of providing an outlet for sewage, other than the river, will rest on the city, and the final consummation of this object be hastened. In either event the people will have gained an important advantage.

WE congratulate Jefferson College, and the medical profession of this city, on the election of DR. MALLET, to succeed the late Prof. Robert E. Rogers. No abler man has held the Chair of Chemistry since the College was founded, and not one who combined more of the requisites for this important position.

Dr. Mallet received his first medical and scientific training at the University of Göttingen, where he graduated Ph.D. Devoting himself to chemistry and cognate subjects, he has achieved a position in the front rank of those devoted to this department of science. He is now a Fellow of the Chemical Society of Great Britain, and also of the Royal Society, the latter the highest and most prized scientific distinction attainable in England, or, indeed, in the world. Dr. Mallet is M.D. of the University of Louisiana, and LL.D. of the College of William and Mary, Virginia. Besides these titles, he is fellow and member of various scientific bodies at home and abroad. He now resigns the Chair of Chemistry in the University of Virginia, to accept the same position in the Jefferson Medical College of Philadelphia.

Dr. Mallet is a fluent and agreeable lecturer, and is a great favorite with students, because he combines an agreeable manner, a lucid style, and a penetrating voice. He is now fifty-two years of age, in the maturity of his powers, and many years of his best working capacity are yet before him. We do not doubt that in his new position he will add much to the reputation of the chair which he fills, and of the city to which he is about to remove.

At the recent meeting of the Canada Medical Association DR. WILLIAM OSLER was elected President for the ensuing year. This honor we learn, from the *Canadian Practitioner*, was tendered at this time, in view of Dr. Osler's probable early transfer to a new field of labor in the States, and with singular unanimity, as a special mark of the esteem in which Dr. Osler is held by his professional associates in the Dominion.

SPECIAL ARTICLE.

THE CHOLERA CONFERENCE AT BERLIN.

(Concluded from page 248.)

PROF. VIRCHOW opened the adjourned discussion upon Dr. Koch's address by saying that, in his opinion, the Conference had been called as a jury to which Dr. Koch should speak, and it was for those present to say whether they were satisfied with his report. In order that the discussion should be kept within bounds, certain questions had been prepared, which covered all the points raised by Dr. Koch's address, and upon which they were requested to express their views.

The first question considered was:

Is cholera generated by a specific infectious material which comes from India only?

DR. KOCH said that he had thought it necessary to bring up this question because it has been doubted on many sides, and especially of late, whether cholera is a specific disease, and originates only in India.

PROF. VIRCHOW believed that there could scarcely be a discussion on this question in Germany; at least he did not know that any noteworthy attack upon the truth of this proposition had been made in Germany in the last decade. No dissenting opinion was expressed.

Is the infectious material contained in the stools, and eventually in the vomited matters, or is it also found in the blood, urine, perspiration, and expired air?

PROF. VIRCHOW said that this question was important because the members of the French Commission in Egypt believed that the infectious material was found in other parts of the body, as in the blood, which was denied by Koch.

DR. KOCH said that, so far as he had seen, the infectious material was found only in the stools, with the exception of one time in the vomited matters. It cannot be contained in the blood, as is shown by the fact that infection never occurs from making an autopsy, whilst with other diseases, as splenic and relapsing fever, in which the infectious material is found in the blood, infection does occur.

Is the presence of the comma-bacillus of diagnostic value (a); and is the infectious material of cholera identical with the comma-bacillus (b)?

DR. KOCH thought that both these points should be considered together, and that (b) covers both. He wished, however, to state expressly that the microscope is sufficient only in a few cases; and that it is especially necessary that culture-methods be employed.

PROF. VIRCHOW said that the practical question is still subordinate. He would state the question (b) thus: Is the infectious material of cholera to be considered as identical with the comma-bacillus? And he would say that some of the elements of certainty were still lacking. On the one hand, cholera has not been communicated to animals through the medium of the comma-bacillus. This, however, is not an absolute counter-proof. Many experiments have been made with the view of communicating cholera to animals. Nor is the necessity of a successful experiment of this kind removed. The experiments of Vincent Richards are well known, but it seems probable, according to Koch, that the disease which he caused was a kind of

intoxication by septic or poisonous material produced by the bacilli. It is certain that in investigations of this kind that comparative experiments should be made on the one hand with pure cultures from the infected animal, and with cholera dejecta on the other. Nor does it yet appear to be absolutely impossible to produce a case of cholera in an animal.

On the other hand, he has expressed himself from the first, that he thinks it very probable that the problem, which has remained in suspense for ten years, as to the existence of a specific microorganism, has now been definitely solved; and further, that of the many microorganisms which are found in the cholera dejecta there is no other one which with any probability can be accused of specificity. Certainly the whole history of cholera, its manner of spreading, shows that that it must be referred to a living organism; and all measures for its prevention must be based on the supposition of this living organism. Since, also, it is entirely improbable that cholera is spread by the transportation of any chemical substance; and since, on the other hand, out of the entire sum of the organisms which are found in cholera only one appears to be in any way peculiar to it, it must be apparent that the probability is extraordinarily great that this one is the peculiar organism of the disease. Koch has reported that in certain dejecta, as also in certain specimens of the intestines of cholera patients, almost pure cultures of this bacillus exist; and that in the soiled linen also, which we recognize as a dangerous means for the transportation of the disease, the growth of these bacilli was very rapid and powerful. From this it seems conclusive that, in a measure at least, the doors are closed against further hypotheses. As to whether this bacillus is present in any other disease or under other circumstances, we cannot yet definitely determine.

DR. KOCH, in referring to the experiments of Richards, recalled the fact that his results could be considered as nothing else than an intoxication. The hogs died in an incredibly short time; in two and a half hours at most. Furthermore, Richards has not succeeded in reproducing his infectious material. He would not assert that there is no animal which could not be affected by the infectious material; he only says that in all those animals which have hitherto been experimented on, and which come in contact with cholera patients, not one has become choleraic. The question, therefore, is still unsettled. But Richards's experiments prove nothing whatever against the signification of the comma-bacilli; on the contrary, they appear corroborative of his opinion as to the etiology of cholera, because one learns in this way that a toxic substance is formed in the intestinal contents under the influence of the comma-bacilli.

PROF. VIRCHOW said that as regards the question, whether the demonstration of the comma-bacillus is of diagnostic value, we know that Dr. Koch is not entirely certain that the diagnosis is assured by simple microscopical proof of the bacillus. We may now ask: If a doubtful case occurs in which the symptomatology points to Asiatic cholera, and microscopic examination should show, without further experimentation, that numberless bacilli were contained in the discharges, would Dr. Koch be doubtful as to the real nature of the case?

DR. KOCH said that in this case he would not be in

doubt for a moment. But it does not often occur that one finds large quantities of the comma-bacilli in a microscopical examination, made with the view of establishing the diagnosis; and, as a rule, culture-experiments are necessary, and should never be omitted. He returned, therefore, to the question as to the practical value of the demonstration of the comma-bacilli; he did not consider it a matter of indifference whether, for example, in the very first cases, which have perhaps been introduced by a ship, or in some such manner, to ascertain certainly whether they are cases of true cholera or not. Prof. Hirsch has said that the difficulty may be avoided by waiting, in order to see whether the cases recover, in which event they are cases of cholera nostras; but men may recover from Asiatic cholera also; and we must wait further and see if the cases spread, in which event the most favorable moment for meeting the disease has passed. Under all circumstances, it is very important that the first cases be taken in hand, and rendered harmless by proper regulations. The diagnosis of the first cases is of great consequence, not only on the very first introduction of cholera into Europe, but also on the appearance of cholera in any place, since then we may render the first cases harmless by isolation.

As regards the difficulty of the methods for recognizing the comma-bacilli, he believed that the coloring of the tubercle-bacilli, which was very quickly adopted, is more difficult than the culture of the comma-bacilli; and furthermore, as very many physicians have successfully colored the tubercle-bacilli, these experiments can certainly be performed by the majority, if not by all, of the health-officers.

DR. SCHUBERT asked in what stage, that is, how soon the comma-bacilli are found; and whether they can be found in the dejecta at the very beginning of the disease?

DR. KOCH stated that he had shown that they were found at the very beginning of the disease. In reply to a question by Prof. Hirsch as to whether, in the case of a man coming to Berlin with the symptoms of cholera, but in whose intestinal discharges no bacilli could be found, he would keep him under observation, Dr. Koch said that he would regard such a man as suspicious. With regard to the necessity of sending cholera material to a special place to be examined, Dr. Koch said that he hoped that it would not be long before every physician would be able to make such examinations for himself.

In reply to Dr. Fränkel, Dr. Koch said that the difficulty of making these examinations was greatly overrated; it is very easy to get the proper kind of gelatine now, and one has only to warm the gelatine, mix it with a particle of mucus from the dejecta, and put it on a glass plate. The plate is then put under a bell-glass, or between two common plates, as he had done in Calcutta. An especial warming apparatus for the culture is not necessary; a summer temperature in the room is entirely sufficient.

Does the infectious material possess great capacity for resistance, and a permanent condition? Is it destroyed in a short time by drying?

PROF. VIRCHOW referred to a late discussion which he had had with Prof. von Pettenkofer on this subject, in which the latter had gone so far as to state the possibility of the material remaining in a permanent form for

a long time, so that the germ of the infection may remain latent in a place for many months and then cause an outbreak of the disease; and he regarded it as very possible that the germ had been introduced into Toulon early in the year.

DR. KOCH said, in reply, that his experience went to show that the cholera germ possessed neither a capacity for resistance nor for remaining in a permanent form. It has been reported that an epidemic of cholera has been caused in a certain part of Switzerland from the sending of rags from Zurich, but the whole matter is so superficially reported that it is entirely uncertain whether all other sources of infection were excluded, that no value can be attached to the case. Certainly, we can refer to endless cases in which rags, soiled with cholera discharges, have been sent out from infected places and no case of cholera resulted. In reply to certain statements of Prof. Hirsch, that there had been intervals of several days or weeks between the first case of cholera and the outbreak of the epidemic in a place, he said that this had no bearing on what he intended by the expression "permanent condition" (Dauerzustand). He now has material from splenic fever which has been dry for twelve years and is still active. We have examples in which syphilis has been inoculated after one year or longer; and we know that vaccine virus remains active for several years. This is what he meant by "permanent condition." But soiled linen cannot remain moist for even a few weeks. He has kept the comma-bacilli in a reagent-glass for six weeks, but they did not on that account possess a permanent form. As soon as they were dried they died immediately. It is noteworthy that on trading vessels, which carry all sorts of articles which could contain the infecting material of cholera, cholera never breaks out except during the first few days after leaving port. But on large vessels it is otherwise; the outbreak may occur soon after leaving port, or it may not occur for two, three, or four weeks, or longer. Now it is strange that the infecting material should remain in a permanent condition only on those ships which have a number of men on board, and get active by degrees. How is it that this never occurs on trading vessels, or those which have only a few men on board, who are not so thickly packed together? This, in his opinion, is one of the most striking examples to show that as a rule the infecting material dies extraordinarily quickly, and that it is only maintained in men by a continuous contagion; which is certainly rendered possible in these extraordinary circumstances by the living together of so many men. The statements which have been frequently made as to the transportation of the disease by the clothes of travellers are therefore very uncertain.

PROF. HIRSCH thought it very possible that in a place in which cholera has become epidemic, the bacillus may be maintained in a moist material when circumstances are unfavorable for its reproduction, and be reproduced again when the circumstances become favorable, and so create a new epidemic; and he referred to the two epidemics in Dantzic in 1848-49.

DR. KOCH replied that this could not be understood as a permanent condition, such as we understand it in other bacteria. It really raises an entirely new question, which should not be confounded with the expression "permanent form," but which is worthy of considera-

tion. He has shown that the comma-bacillus can stand a very low temperature, and that it can exist apart from the human body—on potatoes, for example—or its existence may be prolonged for some time in a reagent-glass with gelatine, or on linen. It is possible therefore that some such thing as this occurred, and that the comma-bacillus underwent a slow growth under restricting circumstances, or held its own for a time without finding an opportunity for infecting. This is always possible, though he has had no such experience, and cannot speak with certainty. To determine this, one must go to a place from which cholera has disappeared for some time, and thoroughly examine all places in which the infectious material could possibly be retained. This could not be done in Calcutta because it is never free from cholera.

Can the infectious material get into the body in any other way than through the digestive canal?

DR. KOCH said that he had purposely introduced this question into the discussion because Pettenkofer is of the opinion that it can also be introduced by the breath and through the lungs.

PROF. VIRCHOW remarked that von Pettenkofer rejects every other method of infection than that by the air and the lungs.

DR. KOCH said that he had found that, under exceptional circumstances, the infecting material can be conveyed through the air, but only to a very short distance; and it may be said, as a rule, that it is not thus introduced. The example to which he referred was noticed in Alexandria, where the sewers discharge into the new harbor; the sea-water was thus soiled in the vicinity, and when standing at the water's edge, one was obliged to clean his eye-glasses about every five minutes when the breakers were high, as they were covered with spray. It is very easily imagined that such spray, from water containing cholera discharges, may carry the infecting germs, and it is the more probable from the fact that a number of the people living on the new harbor were attacked by cholera.

PROF. LEYDEN referred to the fact that the persons who carry cholera corpses are often infected, and said that although they might possibly be infected in some other way than through the air, he thought this hypothesis the more probable; not that the material necessarily reaches the interior of the body through the lungs, but it may be conveyed to the mouth through the air.

DR. SKRZECZKA thought it also possible that in dry weather the infecting material might be carried through the air in the dust, and thus reach the mouth; and that it was also possible that this material may be first dried so that it can be carried about by the wind.

DR. KOCH said that it would be very strange if only the men employed in handling the bodies should be infected, and the nurses and the physicians who come into closer relations with the cholera patients, and with the vaporized discharges, should so often escape. In reply to Prof. Leyden's remark that many of the nurses were attacked, and died, in the epidemic in Dantzic, in 1866, he said that it frequently happens that nurses and physicians are not much more often attacked than other people; they are accustomed to wash and keep themselves clean, but they could not take precautions against the dust. The men who attend to the bodies, however, are

in especial danger, as they go into the dead-house and eat there. But they are not alone exposed to the dust-form of the infecting material, but can be infected in many other ways. To the remark of Prof. Leyden, that he did not mean that the dust was the carrier of the germs, but that he could see no difficulty in supposing that the germs of the infectious material might be suspended in the atmosphere if a strong evaporation was going on, Dr. Koch replied that he must oppose this altogether. All experience with infecting materials and microörganisms is opposed to such a theory.

Is the infectious material reproduced in man, or does this occur independently of the human body in the soil, and is man (and animals) only the carrier?

PROF. VIRCHOW said that there was another question which would come in here. If this is essentially an air-breathing organism, is the human intestine an especially favorable place for its development?

DR. KOCH said that he had already asked himself the same question. But there must be a supply of free oxygen in the intestine, or at least such a combination as will furnish oxygen to the bacillus. We see the bacilli living in great numbers in the intestine, and we see also that if the air is removed from the body they cease to grow. From this it seems that they must find oxygen in some way. It may also be stated that there were large numbers of other bacteria in the intestine, which also stopped growing under the same circumstances. And the *oidium lactis*, which needs oxygen for its development, sometimes grows very richly in the intestine.

PROF. VIRCHOW said that he considered the question as to the soil more important, whether the infecting material is reproduced in the soil, and whether man serves only as the carrier. He thought it very probable that it was reproduced both in the soil and in man. As regards the soil, Dr. Koch has at least shown the possibility that moist earth may serve as a base for its growth.

Is a direct transportation possible, or must the infectious material go through a kind of ripening or growth in the soil or elsewhere?

PROF. VIRCHOW thought that on this point nothing further could be said than what had already been stated; namely, that there is no ground for holding to a hypothesis of a special generation-change or ripening. Certainly, after what had been said, such an assumption would be somewhat arbitrary.

DR. KOCH said that this assumption relates not only to the soil, but also maintains that the infecting material would need a special process of ripening in soiled linen, since the linen cannot be infectious in the fresh state. But the assumption has been especially supported by the experiments of Thiersch, and he was glad to hear it discussed. He himself believes that the soiled clothing is infectious as soon as it becomes soiled.

Is the infectious material conveyed only by human intercourse?

What are the carriers of the infectious material to distant places; ships, articles of clothing, letters, healthy or infected men?

What are the carriers of the infectious material to neighboring places; cholera corpses, cholera effects, soiled clothing, articles of food, water (for drinking and household purposes), the air, insects?

DR. SKRZECZKA said, in reference to these questions, that from Koch's standpoint it did not seem impossible that the contagion could be conveyed by rags and clothing. And it has been certainly proved that by drying for a certain length of time the development of the bacillus and its capacity for spreading are checked. It is, however, not entirely certain what is to be understood by this drying. It is well known that rags are often piled in large heaps, and it seems that a considerable time would be required in order to dry the whole heap.

PROF. VIRCHOW said that rags are no more important as elements for carrying infection than filth; and if, for example, the rags and soiled clothes of a poor man who has died of cholera be thrown into a heap of rags, the possibility of infection is just as great as though the whole heap was composed of soiled rags.

DR. KOCH would only say that he had had no practical experience as to the question now under consideration, and it seemed very doubtful to him. The question of the possibility of infection by means of rags was considered in the Cholera Congresses at Vienna and Constantinople, and no one could bring forward a single example of cholera being spread from rags, as, for example, amongst the rag-sorters in paper mills; and it must be concluded that the handling and working of the rags is such that the infectious material is destroyed. And of what avail would it be to keep rags out of the country while men infected with cholera are allowed to come in? The possibility of the transportation of the infection by rags is, in the light of experience, infinitely small, whilst that by apparently healthy men is very great. In answer to a question by Prof. Virchow, he said that in his experience the comma-bacillus dies quite soon in clear water, that is, within a few days.

Is a special individual predisposition necessary for the infectious material to become active?

How long is the stage of incubation?

Does recovery from cholera confer immunity from the disease for any definite period?

Can the action of the bacilli be regarded as a kind of intoxication?

PROF. LEYDEN said that one attack of cholera seemed to give a certain immunity against a second; but that this immunity is not absolute. Persons have been known to have three attacks of cholera in one epidemic, but it is seldom that a person is twice attacked in two epidemics separated by a short interval of time.

DR. KOCH, in answer to a question by Prof. Hirsch, said that he had found out nothing further regarding the period of incubation than had already been stated in his report.

PROF. HIRSCH said that he had never known of a period of incubation of less than two days, and that, as a rule, it was from three to four days—never five—as he learned in 1873 on his cholera expedition.

PROF. VON BERGMANN said that it seemed important to him that, for the prophylactic measures which must be employed, we should be able to make use of Dr. Koch's experience as to the spread and destruction of the infectious material. To the answer of Dr. Koch, that this would involve, what they wished to avoid, a determinate judgment as to the entire conception of cholera, and that each one should form his own opinion from what had been said in the discussion, Prof. von

Bergmann replied that each one could do this, but it was important to protect commerce and the community. As to what had been said, it gives a good deal of latitude to the physician. As regards the experiments on drying, he would propose that certain disinfection regulations be tried beside those which have been already used. All possible materials should be tried, and disinfection with different antiseptics and in different places.

PROF. VIRCHOW then announced that the discussion was closed, and said that he had some hope that they would meet at another time in order to hear of new experiments, and to discuss other points. That would of course depend upon the course of events and especial circumstances. He then expressed his thanks to the members of the Imperial Health Office, and especially to Dr. Koch, who had not only welcomed the visitors, but had imparted so much new and important knowledge to them.

CHOLERA NOSTRAS AND THE COMMA-BACILLUS.—The *Deutsche medicinische Wochenschrift*, of August 21st, states that during the past few days there have been a number of cases of cholera nostras in Hamburg, which ran a rapid course. Careful microscopical examinations were made of specimens from the last fatal case, and culture-experiments were also made, with the view of finding the comma-bacilli, but with completely negative results.

REVIEWS.

THE HIP AND ITS DISEASES. By V. P. GIBNEY, A.M., M.D., Professor of Orthopædic Surgery in the New York Polyclinic, etc. 8vo. pp. iv. 412. New York: Bermingham & Co., 1884.

THE author of this book has had unusual opportunities for the study of joint-affections, having for thirteen years resided in the Hospital for the Ruptured and Crippled, in New York, during which time he has seen over two thousand cases of hip-joint disease alone—an astonishing number to come under the observation of one man. It need hardly be said that conclusions deduced from the study of so large a material are worthy of the utmost consideration; and although our space will not permit a critical analysis of them, we will mention a few of the most important.

In the first place, we note the author's statement that the majority of cases of hip-joint disease occur before the eighth year, and are the consequence of an osteitis interna, the focus of the disease being in or near the centres of ossification; that the head and neck of the femur are oftener involved than the acetabulum; that synovitis is secondary to the osteitis, and is simple if developed by contiguity, and purulent if by perforation; while after the age of eight years the origin of the disease is a central osteitis, a periostitis, a chondritis, or a synovitis, with about equal frequency.

In regard to the etiology of joint-disease, the author's experience leads him to divide the cases about equally between those in which the disease depends upon an inherited strumous diathesis, and one in which this has been acquired. One of these, however, he regards as the great predisposing cause of all chronic inflammatory

bone lesions of the hip. He thinks the disease may be excited by a traumatism, exposure to cold, acute disease—an exanthema, for example, and most frequently—while in many cases no exciting cause can be discovered. He has about equally little faith in the way in which an exciting cause is often decided upon, and the way in which an inherited diathesis is decided against.

In regard to the matter of treatment, Dr. Gibney favors a method which is somewhat eclectic. The expectant method—that is, the symptomatic, if it may be so expressed—he thinks will secure good results in very few cases; in the large majority it is utterly inadequate to arrest the disease or secure the best result. For this reason it is to be rejected whenever the diagnosis of chronic osteitis of the hip is certain; when there is doubt, it may be adopted till the case becomes clear. The so-called physiological method of Dr. Hutchison—the crutch and high shoe—he thinks a delusion, securing only temporary improvement; though it has a certain value as part of the expectant plan of treatment. His preference is decidedly for fixation-apparatus; extension he does not seem to have much faith in. Short splints, which permit motion, are of little use, in his opinion. The long splint secures the best results in competent hands. The only basis for the proper treatment of hip-joint disease, he holds to be the idea that this is not a synovitis, but a disease of the bone-substance, requiring the same principles for its management as a fracture, namely, fixation and rest.

As a whole, it may be said that this book is worthy of most careful study. It has some defects of style, and is opposed to the convictions of some who think better of certain methods of treatment than the author does. But it is full of suggestions, and exhibits a thoroughly candid, though fearless, spirit in dealing with matters of dispute. In fact, its excellences are so many that justice cannot here be done to them all, and its defects so few that it would be hypercritical to dwell upon them.

SOCIETY PROCEEDINGS.

AMERICAN DERMATOLOGICAL ASSOCIATION.

Eighth Annual Meeting, held at Highland Falls, N.Y., August 27, 28, and 29, 1884.

(Specially reported for THE MEDICAL NEWS.)

(Concluded from page 271.)

AUGUST 28TH, SECOND DAY—MORNING SESSION.

DR. L. A. DUHRING read a paper on

DERMATITIS HERPETIFORMIS AND ITS RELATIONS TO IMPETIGO HERPETIFORMIS;

in which, after describing the disease and its several varieties, he endeavored to show that the disease of Hebra was merely one manifestation of an extensive multiform and protean process. The history of impetigo herpetiformis was referred to, and a *résumé* of all the reported cases given, the conclusion being arrived at that the so-called impetigo herpetiformis of Hebra should be viewed as the pustular variety of dermatitis herpetiformis.

DR. FOX thought great credit was due Dr. Duhring for taking so comprehensive a view of many varying forms of disease which had hitherto been described under so many different names, as pemphigus pruriginosus, herpes chronicus, herpes gestationis, etc. He thought that all these belonged to the disease which Dr. Duhring had described.

DR. J. C. WHITE was convinced that the more we studied these cases the more difficult would we find it to classify them absolutely. He was inclined to think that the term multiform dermatitis would apply to them better than the others which had been suggested. He was not quite ready to admit that the writer of the paper had shown the existence of an absolute connection between his own cases and those which Hebra described as impetigo herpetiformis. He had never seen a case of Hebra's fatal impetigo of pregnant women, and would therefore prefer to regard the question of the identity of the two affections as still undecided. He saw no more reason for regarding the affection described by Duhring as of a neurotic origin than in almost any other inflammatory affection of the skin. Pigmentation was rare in the cases that he himself had seen, and the affection did not recur. He called attention to the fact that the disease was much more likely to be confounded by the general practitioner with eczema than with herpes.

DR. W. A. HARDAWAY thought that a certain class of cases in which the lesion assumed the form of vesicles, papules, and pustules could properly bear the term "herpetiform." He thanked Dr. Duhring for grouping these and giving them a perfectly satisfactory name. He thought there was a neurotic basis to many of these affections, but he would not regard it as a disease any more than he would call a drug eruption a distinct disease.

DR. A. R. ROBINSON spoke of three cases which he regarded as belonging to the class described. He felt that thanks were due Dr. Duhring for classifying these cases, which all dermatologists had seen and were at a loss to designate. He also was not willing to admit, without further investigation, that they were identical with Hebra's cases of impetigo herpetiformis. He had never seen the scarring described by Dr. Fox as occurring in the disease.

DR. S. SHERWELL said that he believed Hebra's cases to have been of a septic origin, identical with those often seen in military hospitals infected with hospital gangrene. He himself had seen many such in the ambulance at Sédan under the care of Sir Wm. MacCormac. He was inclined to suggest the name of "dermatitis necrogenica" for such cases, believing that even in Hebra's cases in pregnant and puerperal women, there might have been a septic absorption from an occluded tube in salpingitis, from the peritoneal cavity, a uterine clot, retained placenta, etc. The pruritus so common in the cases he was induced to regard as the result of the irritation from the deposits in the skin.

DR. DUHRING, in closing the discussion, said that his paper was practically a synopsis of one read at the last meeting of the American Medical Association, which was not yet in print. His information concerning the disease was based upon the careful study of sixteen or seventeen cases of the affection observed during the past fourteen or fifteen years. He did not regard the

objections raised against the term "herpetiformis" as valid, there being at first in many of the cases a distinctly herpetic character. He admitted that most of his cases were of a different type from those of Hebra, but some of them closely resembled the latter, except that they did not prove fatal. A few of them were so acute as to present cutaneous disturbances of the most intense grade. He thought the difference between the two sets of cases simply one of degree. He thought that most of his cases had a neurotic origin. In some cases the disease is undoubtedly septicæmic, as it doubtless was in Hebra's cases. The causes of the disease were various. In some of his cases the affection was both neurotic and septicæmic, and he would soon report a case in which the disease was caused by a nervous shock, it having appeared on the next day. In his experience pigmentation was common, some of the cases resembling mulattoes. The disease was unquestionably a distinct affection, as much as any skin disease. He thought the term "necrogenica" decidedly objectionable, as in many cases there was not a possible suspicion of a septic origin.

AUGUST 29TH, THIRD DAY—MORNING SESSION.

In the absence of Dr. Henry W. Stelwagon, the Secretary read two papers forwarded by him entitled: *A Case of Late Cutaneous Syphilis (Acne-form Syphiloderm of the Nose), Illustrating the Occasional Necessity of Large Doses of Potassium Iodide*; and *A Case of Vitiligo Involving the Whole Surface*.

DR. W. A. HARDAWAY then read a paper on

A CASE OF GENERAL IDIOPATHIC ATROPHY OF THE SKIN.

The patient was a blind man, aged twenty-three years, of healthy parentage, and having a sister who was also blind and, he said, suffered from the same skin disease as himself. His trouble dated from infancy, and he had had eye-trouble for sixteen years. With the exception of headaches, his health was good. His intelligence was good, but he was poorly nourished. His face presented a rosaceous appearance, the skin being thickened. No papules or pustules were present. There were scars around the mouth, and his lips were thickened. The skin of the neck was pigmented in spots, and red on the sides, showing enlarged vessels. Front of trunk presented a shining, checkered aspect, due to many pigmented spots of various sizes and atrophic macules. The whole skin, in fact, was atrophied and tense. There was a number of cicatrices over the chest and abdomen, and the umbilicus was on a level with the surrounding skin. The back presented the same aspect as the front of the body. No telangiectases were at any time visible. The skin around the elbows was deeply pigmented and slightly scaly. The skin and muscles of the hand were atrophied, and the sides of the fingers were adherent to each other as far as half way up to their tips. The condition of the lower limbs resembled that of the upper. The feet were about normal. The hairs were short and scanty all over the body. The perspiratory function was poorly performed, but the cutaneous sensibility was not much altered. There were no marks of subjective symptoms. Examination of the eyes showed necrosis of the conjunctivæ, adhesion of the lids to the globes, and opacities of the corneæ.

DR. L. A. DUHRING said that his study of the recorded cases of this disease had taught him that beyond atrophy there were no special features which distinguished the process, neither telangiectasis nor pigmentation being essential elements. In the scleroderma of Hebra and the angioma pigmentosum et atrophicum of Taylor the features presented often varied greatly. We meet with cases of the disease in all grades, from those having no tendency to degeneration to those developing sarcoma or carcinoma, as in the graver forms of the affection.

DR. R. W. TAYLOR gave it as his opinion that the condition of the skin in the case reported was not like that met with in angioma pigmentosum et atrophicum, this patient presenting simply an ill-nourished, senile-like condition of the integument, with no evidence of vascular disturbance. The process had not been active enough to lead to neoplastic formations. In cases showing these, they usually appeared at the junction of skin and mucous membranes, or upon surfaces constantly exposed to irritation or friction from active motion.

DR. J. C. WHITE was unable to agree with Dr. Duhring as to the affinity which he believed to exist between the different varieties of atrophic processes in the skin. In one class of cases we met with loss of pigment and a deposition of epithelium, without change in the corium, and in another hypertrophy of the corium was a marked feature. In still another class of cases, new growths, such as sarcoma and carcinoma, were present. He did not think that the case reported could be connected with these. In the majority of the cases he had alluded to, the atrophy was a mere phenomenon, and not the essential feature of the disease.

DR. A. R. ROBINSON then read a paper on

MILIARIA AND SUDAMINA.

His object was to attempt to define clearly the true nature of the affections, and the conclusions he had reached were largely based upon careful microscopical studies of the diseases. Section through the deep-seated vesicles (which usually occurred on the face) showed that the papular or vesicular formation consisted of an exudation into the rete mucosum around the orifice of a sweat-duct; composed of serum, corpuscles, and more or less sweat, but never of the latter fluid alone. The changes in the rete were similar to those encountered in eczema, and the exudation came from the vessels of the papillæ. A catarrhal condition of the sweat-glands was also often noticed. He concluded that miliaria alba and rubra were not inflammatory processes of the sweat-glands, but rather of the skin around them. Specimens of sudamina had revealed the fact that the vesicles were always formed in the corneal layer, and that they were in direct communication with sweat-ducts. The surrounding rete mucosum was normal, and the contents of the vesicles were therefore sweat. The glands and ducts seemed normal. The disease was therefore not an exudative process, but consisted in an increase in the sweat-formation from hyperæmia with some obstruction to the escape of the sweat; he therefore concluded that miliaria alba and rubra were forms of eczema, or catarrhal inflammation of the skin, due to heat, irritating clothing, acrid sweat, etc. That the affection should not be called sudamina was shown by the fact that the contents of the vesicles were not

sweat, but of a serous nature. The dew-drop like eruption was not an exudative affection, but a disturbance of the sweat-glands, the result of an hyperidrosis. He thought that the term miliaria should be dropped, as the affection was of an eczematous nature, and that the term sudamina should be applied to it.

DR. W. A. HARDAWAY objected to the use of the term eczematous as applied to the affection under consideration, as the latter was a true dermatitis and eczema was a distinct disease.

DR. J. N. HYDE was inclined to accept fully the views advanced in the paper. He thought that miliaria and lichen tropicus were simply due to accidents occurring in the course of an eczema.

DR. J. C. WHITE thought that any attempt to lessen the confusion in dermatological nomenclature should be warmly welcomed; and also that the position taken by the author was the only philosophical one. He himself recognized no such diseases as miliaria alba and miliaria rubra as distinct affections, but regarded them as merely forms of eczema. The question was, after all, one of definition only.

DR. L. A. DUHRING held that miliaria was a disease characterized by true inflammatory phenomena, such as papules, vesicles, and vesico-pustules, beginning around the sweat-glands and ducts. It seemed to him very plausible that the products of this inflammation should show themselves at some distance from the glands, and not at their orifices. He had always held that the disease was primarily caused by some disturbance of the sweat apparatus, resulting in vesiculation or papulation or diffuse hyperæmia of the general surface.

DR. ROBINSON called attention to the fact that the sweat-glands stop at the upper part of the corium, there being no perspiratory apparatus in the epidermis. In the so-called miliaria the formation of papules or vesicles takes place around the sweat-orifices, which proved that the disease was not an affection of the glands. The point of greatest hyperæmia is around the orifices of the ducts, and if this should be prolonged a quasi-inflammatory process would result, while if the irritation were not too prolonged or intense, vesicles or papules would be formed around the congested spot as a *pars minoris resistentiæ*, as it were, without the sweat-glands themselves being in the slightest degree involved. Hyperidrosis alone never gave miliaria without the influence of some external irritation. He thought that as we had dropped the old term for this affection, lichen simplex, we might just as advantageously drop miliaria.

DR. R. W. TAYLOR then read a paper on

A PECULIAR RINGED AFFECTION OF THE GLANS AND PREPUCE.

He described three cases of the affection seen during the past ten years. The patients were all from forty to fifty years of age, and all free from syphilis. Marked neurotic disturbances preceded and attended the outbreak in all the cases, in one an attack of creti-epididymitis and recurring attacks or balanoposthitis, and in another a chancroid had been severely cauterized on the glans, which led to long-continued swelling and other inflammatory disturbances. In all the cases the disease appeared in the form of a few rings covered with thin, firmly adherent, glistening scales, seated upon a normal mucous membrane without hyperæmic halo.

The lesions always began as a ring, and not as a papule spreading peripherally. After the lesions had appeared they remained stationary, showing no tendency to extend, and no new ones coming out. The affection was confined to the mucous membrane, the adjacent skin not being involved in any of the cases. Disagreeable sensations in the affected and adjacent parts were present in all of them, and the author of the paper had recognized the existence of a marked neurotic element in all. The affection resisted obstinately all local treatment, and would grow better and worse in a very capricious manner. The cases all finally recovered under the long-continued use of large doses of arsenic. The first case has since remained free from the disease for six years, the second five. He was loath to regard the disease as one *sui generis*, but in studying the cases he had carefully eliminated syphilis, tinea circinata, and psoriasis from the diagnosis.

DR. L. A. DUHRING said that, as far as the gross appearances of it went, he was very familiar with the diseases described. As regards the subjective symptoms, they were absent in his cases. These were also three in number. He was very much puzzled when he first saw them, but had finally concluded to call them lupus erythematosus. After listening to the paper, he felt satisfied that the cases were instances of the same affection Dr. Taylor so graphically described. In his cases the appearances, the course and evolution of the lesions, the occasional outbursts of hyperæmia, and the persistence of delicate very superficial cicatricial tissue here and there after the disappearance of the congestion, all pointed to lupus erythematosus. The course of the disease was very variable, the rings at times shortening from central pin-head-sized papules, and increasing to one-half inch in diameter, often coalescing and clearing up in the centre and scaling off. At times the lesions took the form of patches, not rings. They were seated upon the glans, extending over the corona and spreading over the mucous membrane, half on the corona and half on the prepuce, being most marked on the former. These cases were free from neurotic manifestations, and all proved very obstinate. A cure was finally accomplished in two of them by destroying the lesions with caustic potash.

DR. J. C. WHITE had had one case under observation for some months. The glans alone was involved, over almost its entire surface. The disease had existed a year, and was no better when last seen. He had made no positive diagnosis, but thought that it might have been lupus erythematosus.

DR. W. A. HARDAWAY described what he regarded as a similar case. The patient had been treated for psoriasis and recovered. A few months later he presented himself with a red, scaly, not thickened nor elevated patch on the side of the glans penis. No improvement had occurred after six months' treatment, even with increasing doses of arsenic. The course of the affection was entirely unlike that of psoriasis.

DR. W. T. ALEXANDER, of New York, then suggested that perhaps the affection might partake of the nature of the disease known as leukoplakia buccalis. He thought there was a marked resemblance between the clinical histories of the two affections.

DR. E. WIGGLESWORTH, of Boston, cited the case of a man whom he had successfully treated for leukoplakia

buccalis, and who afterwards returned with a kidney-shaped patch, clearing up in centre, on the glans penis. It was attended with disagreeable itching sensations. There were no evidences of syphilis in the case, and he had eliminated from the diagnosis everything but lupus erythematosus, an instance of which affection he finally pronounced it. It finally disappeared under the use of pure carbolic acid.

DR. TAYLOR, in closing the discussion, said that in his cases the affection did not begin as a papule, but always as a ring, clearly defined, as large as a ten cent piece. They were not indurated, were slightly elevated, thinly covered with dull-gray adherent scales. No scarring had taken place. There were also no attacks of recurring hyperæmia, and he was confident the cases were not instances of lupus erythematosus.

The following were then elected

OFFICERS FOR THE ENSUING YEAR:

President.—Dr. W. A. Hardaway, of St. Louis.

Vice-Presidents.—Drs. J. C. Graham, of Toronto, and A. Van Harlingen, of Philadelphia.

Treasurer.—Dr. G. H. Rohé, of Baltimore.

Secretary.—Dr. W. T. Alexander, of New York.
Mr. Jonathan Hutchinson, of London, England, was elected an *honorary member*.

The following were chosen to *active membership*:

Dr. Geo. H. Tilden, of Boston; Dr. A. R. Morison, of Baltimore; Dr. F. C. Curtis, of Albany; and Dr. L. N. Denslow, of Minneapolis.

The Association then adjourned to meet again on the last Wednesday in August, 1885, at the Indian Harbor Hotel, Greenwich, Connecticut.

CORRESPONDENCE.

THE INTERNATIONAL MEDICAL CONGRESS.

SECTION OF GENERAL PATHOLOGY AND PATHOLOGICAL ANATOMY.

(From our Own Correspondent.)

COPENHAGEN, August 17, 1884.

THE Section was opened under the presidency of Prof. REISZ, of Copenhagen, at 10 A. M., August 11th.

The first paper was entitled *Necrosis by Coagulation*, and was read by PROF. WEIGERT, of Leipzig.

Necrosis by coagulation is a process in which tissues are transformed into a substance resembling coagulated albumen; this change has been compared to that forming fibrin and cheese. The change, however, was not explained correctly, being confounded with those in the fibrin of the blood, or in which cheesy change occurred with desiccation and inspissation of the dead tissues. The parts coagulated should be dead, and also have the signs of coagulation from a macroscopic aspect; microscopically this change is characterized by the loss of the cell-nuclei, which, however, may also occur in putrefaction. The following conditions are necessary for this coagulation, viz.:

1. The organs must contain substances capable of spontaneous coagulation, as in all the protoplasm.
2. They must be dead—the coagulation is the *result*, not the *cause*; the latter may be ischæmia, trauma, chemical agents, or microorganisms. The victims may

be either whole organisms (extrauterine pregnancy), or parts (infarcts, diphtheria, cheesy parts of tuberculosis, abdominal typhus, tumors, etc.), or cells (necrosis of kidney epithelium or leucocytes).

3. The dead and coagulated tissue must be in intimate connection with plasma fluids; in order to be this they must be either in the interior of the organs or supplied with living vessels.

4. Agents opposed to coagulation (putrefaction, purulent poisons) must be absent. The fate of these coagulations is softening, calcification, and formation of hyaline substances (waxy degeneration, coagulation of renal epithelium). This hyaline is, however, not a special product.

Macroscopically the tissues resemble the normal ones.

PROF. VIRCHOW differed from Prof. Weigert's views, which he declared were somewhat obscure. He objected also to the name, which might lead to the belief that the death of the tissues is brought about by the coagulation; a better name would be coagulation of mortification. He would rather call it an *Inspissation*. The main question appears to be whether the substance was originally fibrinous material or not; in the latter case, it will not turn to fibrin. The first act in a coagulation is the loss of liquid, which is independent of plasmatic fluids. The fluid does not penetrate, only the water in the tissues is not held, owing to a loss of vitality (plants, infusoria). The retention of liquid in tissues is an act of vitality. We do not know the reason, but believe that the loss of liquid is a sign of lessened life. He does not believe that the change of tissues to the hyaline substance necessitates the absorption of liquid (death of echinococcus). In the dead fœtus also the first occurrence is the absorption of the amniotic fluid. There is no coagulation, but an inspissation; no fibrin is formed. The intercellular spaces disappear more and more, and the external parts may possibly be subjected to a fatty degeneration. The interior parts, however, do not change (*vide* a placenta which he had seen, which had been forty years in the uterus). A parallel might be found in a coagulation of blood found in muscular hemorrhage, where the blood finally is converted into a cheesy mass formed of a compact agglomeration of blood-corpuscles (he had seen it five years old). This is surely an inspissation, no coagulation or new substance.

Regarding the changes in the cells, we find a similar one in carcinoma, when they shrink, the tissue then becoming denser and presenting a homogeneous appearance. He admitted a chemical change in cheesy metamorphosis different from lymph. He could not see any progress in Prof. Weigert's explanation. His view would be important if the process commenced with coagulation, and was followed by death. It would also make a great difference, if the muscles disappeared first and not after the death of the tissues.

PROF. WEIGERT said the name was suggested by Prof. Cohnheim. He had avoided the word fibrin in describing this coagulation, because fibrin is a chemical term. He was totally opposed to the explanation by loss of liquid.

PROF. VIRCHOW used the word inspissation.

PROF. WEIGERT might admit partially this word. The loss of liquid, however, is not the primary act, as it would be followed by a loss in size; which is not the

case (caseous degeneration of lymphatic glands). In tumors which undergo cheesy degeneration or cheesy pneumonia the cells or affected parts are not smaller than before the change. The loss of liquid or size is a subsequent occurrence, which is independent of the coagulation. The nuclei do not disappear in mummification either.

PROF. VIRCHOW remarked that cheesy glands diminish in size.

DR. C. J. SALOMONSEN, of Copenhagen, read a paper on *The Effects of the Infusion of Jequiritin*. The ophthalmia caused by jequirity is the result of a chemical poison, which probably belongs to the group of "soluble ferments." The active principle has received the name of jequiritin. The bacteria which are always found in the jequiritin infusions prepared without special precautions have no importance in the causation of the ophthalmia. Such an "ordinary infusion," injected into the dorsal sac of a frog, other conditions being equal, causes death, the time of this fatal result being in direct ratio to the strength of the solution. A "sterile infusion"—*i. e.*, devoid of bacteria, has the same result. The blood of frogs killed by the former infusion is always filled with an enormous quantity of bacteria, while with the latter solution their number is very small. These bacteria do not belong to any particular species, and in the same frog different species may be found. They are not virulent, and injected into the dorsal sac of the frog, or instilled into the conjunctiva of the rabbit, they produce no sensible result. The principle which causes the death of the frog is the same which affects the conjunctiva of the rabbit—*i. e.*, the jequiritin. The blood of frogs which have been killed by a strong infusion of jequirity, produced on injection into the frog a fatal jequiritin intoxication, and with the rabbit the usual ophthalmia. The same does not obtain when the blood of a frog killed by a weak infusion is used. The numerous non-virulent bacteria which are found in the blood of frogs killed by an ordinary infusion originate in this, and by using special care in selecting the bacteria any species of bacillus may thus be multiplied without affecting the final result. But they are not virulent unless mixed with an infusion of jequiritin, and do not multiply in the sac of a normal frog, which has the faculty of eliminating them. The limited quantity of bacteria found after the use of a "sterile infusion" originates probably from the accidental introduction through the liquid covering the frog at the time of inoculation; they do not seem to preëxist in the blood. Dr. Salomonsen's conclusion is, therefore, that after the injection of an "ordinary infusion" the frogs die from a poisoning by jequiritin accompanied by a considerable invasion into the blood of non-virulent bacteria; a poisoning complicated with pseudo-infection and simulating an infectious disease. Rabbits, mice, and chickens killed by an injection of jequiritin, therefore, die from jequiritin poisoning, and not from a disease of bacterial nature. Neither the blood, nor the products of inflammation in these animals have any infectious qualities.

PROF. CORNIL, of Paris, remarking upon these views, said there was no absolute proof of the existence of an alkaloid such as jequiritin. He found it very difficult to make an absolutely sterile infusion, but succeeded by

filtering through porcelain, and found no bacilli under the microscope. This infusion he injected without producing any toxic results. If the active principle was soluble in water, the effect should have been obtained. The substance left in the filter, however, was found filled with bacteria, and caused intoxication by injection. These bacteria he had cultivated, and even in the fourth culture obtained toxic symptoms. The sterile liquid, after being exposed to the air, caused death within twenty-four hours. He heated a jequirity maceration, when it became non-virulent, but on adding bacteria it became toxic. He, therefore, believes that the bacilli are the active principle.

DR. SALOMONSEN replied that he had not said the bacilli had nothing to do with the intoxication, only that they were innocuous in a *sound* animal; as soon as the fermentative action of jequirity has taken place they become toxic. As to jequiritin being only a name, he admitted that, like all soluble ferments, it is not chemically well defined. Regarding the filtration, the ferment might be retained on the filter as it is in dialysis, hence the filtered infusion might be devoid of it; consequently that argument is not conclusive, to establish it it would be necessary to obtain a sterile liquid, filter it, and then fail.

The next article was on the *Staining of Bacteria in Sections*, by DR. CHR. GRAM, of Copenhagen. The purpose of the paper was to show that the bacteria of pneumonia and typhoid fever can easily be stained by dipping them in a concentrated solution of gentian violet, and then fixing the color in a one per cent. watery solution of corrosive sublimate. He reported that Dr. Friedlander succeeded in obtaining excellent staining of sections with coccobacteria by immersing them in a solution of 1 part of fuchsin, 100 of distilled water, 5 of alcohol, and 2 of glacial acetic acid, after which they are washed first in alcohol, and then during two minutes in a two per cent. solution of acetic acid, subsequently cleaning them as usual in alcohol and oil of cloves.

PROF. MALHERBE, of Nantes, then read a paper on the pathological anatomy of *Painful Subcutaneous Tubercles*. This disease is rare, he has only seen it five times in ten years (syn. painful fibroma). The tumors are from the size of a lentil to that of a filbert, and are noticed by the extreme sensitiveness to touch, which gradually turns into a spontaneous neuralgia. Their usual seat is in the vicinity of the malleoli and the knee, and they generally occupy the deeper layer of the skin, or the subcutaneous tissue. Anatomically they are composed of organic muscle-fibres, crossing each other in every direction, encased in a thin capsule. No nerve-filaments were discovered. Extirpation gives immediate and permanent relief.

PROF. PASTEUR, of Paris, in general session, read a paper on *Morbific Microorganisms and Vaccinia-matter*. He was most enthusiastically received. He first referred to the advances made in the study of microorganisms since the last Congress. Three years ago, at London, no one thought of believing in the microbic origin of contagious diseases. From that time also dated the doctrine of attenuation of virus by the culture of the microbes, the discovery of the anthrax vaccine, and the cholera bacillus. Four years ago he commenced his studies of hydrophobia, which, owing to

the length of incubation in the animals operated on, became protracted. He would give to-day his results. Of course, in a dreadful disease like hydrophobia, the first thought was to find a remedy, but in order to discover this the nature of the disease had to be known. Numerous dissections of dogs which had succumbed to the disease, showed that its seat was in the brain and spinal cord, but more especially in the medulla oblongata, this latter being constantly affected. Foci of inflammation were found in different parts of these organs, and were in direct proportion to the severity of the case. The principal seat of the poison is in the subarachnoidal space. The arachnoid liquor, which never fails in inoculation, was used for experimental purposes, and trephining employed to lay bare the dura mater, under which the virus was injected by a Pravaz syringe. This is the only absolutely certain mode of inoculation. Thanks to this positiveness of results, he could advance with his experiments on a sure basis. This mode of inoculation also shortened the period of incubation, a matter of great moment in such lengthy experiments. The duration of incubation was found to be in direct proportion to the quantity of virus introduced.

Here Prof. Pasteur gave the results of numerous experiments, mentioning each, a relation of which would prolong this account too much.

He found that in rabbits, after subarachnoidal inoculation, the time of incubation at the first injection varied from fifteen to twenty-one days, never more and never less. Virus taken from the first rabbit would lead to rabies in a second in twelve days, the next ten, the next eleven, until the twenty-first inoculation, when to the fiftieth there was an absolutely fixed incubation of ten days. Guinea-pigs have an incubation of five days—five or six cultures lead to the maximum of virulence. Monkeys have an incubation of twenty-three days, virus obtained from them caused a longer incubation in the rabbit, and by passing a number of times from rabbit to monkey the activity of the poison, instead of increasing as it did in direct transmission in the rabbits, became lessened and the time of incubation lengthened. The microbe of hydrophobia has not yet been discovered.

He then gave an account of his last great test of the efficacy of this attenuated virus. A distinguished commission was assembled in Paris to examine into his experiments, and to these gentlemen he delivered twenty-three vaccinated dogs, sixteen of which had been vaccinated by trephining, the remainder hypodermically. These dogs were at different times shut up with rabid dogs and were frequently bitten—a certain number of unprotected dogs were treated likewise. All these experiments were made a short time ago. The result was that of the vaccinated dogs none suffered any ill-effects, while already four of the unprotected dogs have died from rabies.

This is only a sketch of a discourse which lasted an hour and a half, and was followed with the closest attention and frequent loud applause.

The work of the Section was resumed on Tuesday, at 10 A.M., with a paper by PROF. RAYMONDAUD, of Limoges, on the *Ixodes, a Parasite but Little Known in the Human Body*. He exhausted the natural history of the different species of ixodes (tick), and related a few cases in which the bite of this arachnid was followed by

ulceration, and in one case it was supposed to have caused death. The paper was very exhaustive, but scarcely belonged to the class of subjects which one would expect to find discussed in such a galaxy of pathologists as were present.

The discussion was opened and closed by Prof. Virchow, who, with his incisive humor, remarked that the animal was very common in Germany, that when it had fastened in the skin it was lifted out with a needle, and that he had never heard of any ill-effects resulting.

This paper was followed by one from PROF. HELLER, of Kiel, on *The Heredity of Tuberculosis*. He said that the views held up to the present time, that tuberculosis can be transferred to the fetus at the moment of fecundation or during its uterine life, is no longer tenable. The proofs heretofore adduced in favor of the old theory can be explained otherwise. There is a possibility of this transmission, but facts speak against it. He based his view on observations made in 1300 cases. Tuberculosis is, if it ever occurs, very rare in the fetus and the new-born child up to the end of the first few weeks. It appears only toward the end of the second month after birth, and its frequency rapidly increases from that time. The mode of its conception speaks against the supposition of intrauterine origin. When apparent cheesy degeneration of the lungs was observed in the fetus, it was generally found to originate from hereditary syphilis.

A certain predisposition, however, must be admitted in the progeny of tuberculous parents, but it has been contended that when the child was not allowed to nurse from such a mother, the chances were in favor of its immunity.

Prof. Heller then related a case of pregnancy in a tuberculous mother, who died before giving birth. The fetus was perfectly sound—the mother suffered from general tubercular degeneration of all the organs, even the uterus was degenerated, and a preparation, exhibited under the microscope, showed large quantities of tubercular bacilli in the epithelial cells.

The discussion was again opened and maintained by PROFESSOR VIRCHOW. He said that thirty years ago he had disputed the heredity of tuberculosis. He, however, admitted the predisposition. He had not seen a single case of direct transfer in the fetus; where there seemed to be such an appearance, it would only be an isolated case of spontaneous origin. He would like to have a closer definition of the term predisposition. Thirty years ago it was explained on the principles of humoral pathology, but he did not attribute it to the blood—rather to a lessened power of resistance in the tissues. He desired to talk again on the subject. He believed that the key could be found in the peculiarities of the lymphatic system, which varies in every individual. It evidently, under the influence of this weakening of the system, deteriorates from one generation to another, until, as we have often seen, whole families are wiped out of existence; fatal results appearing at a more and more early age.

PROFESSOR HELLER remarked that he had not denied the heredity of the *predisposition*, only the direct transmission of the disease to the fetus. He believed, however, that the mucous membranes have more to do with the susceptibility to the infection, and that persons with rapid changes in their mucous membrane will be

more apt to throw it off than those whose membranes are not active.

The next paper read was by DR. GRANCHER, of Paris, on the *Relation between Scrofulosis and Tuberculosis*.

PROF. PASTEUR here entered unostentatiously, but was vociferously applauded, and conducted to the presiding chair.

After giving a historical review of the history of scrofula and tuberculosis from the sixteenth century to the time of the doctrines of Virchow and Borzin, in 1860, Dr. Grancher showed that by the aid of pathological histology and the knowledge of the different phases of evolution of the tubercle, the domain of tuberculosis has been enlarged at the expense of scrofula, and has absorbed fungous osteitis, lupus, and cold abscess. The discovery of the virulence of the tubercle and of the tubercle bacillus has completed the demonstration.

There is nothing left to scrofula but a few skin diseases like eczema and impetigo, and a few diseases of the mucous membranes which scarcely display more than a morbid disposition. The researches of Dr. Grancher were mainly directed toward establishing the question if the discharge of these skin diseases, especially impetigo, or inoculation would produce the changes of tuberculosis. The experiments were made on the guinea-pig, and were negative, while in every instance control-injections of tubercular matter produced tuberculosis. He related twenty-three experiments, twenty-one of which remained negative as far as discharges derived from impetigo and mucous secretions were concerned; in two cases, however, caseous degeneration took place, but it must be remarked that in these cases the virus was not taken from the impetiginous discharge, but from an adjoining small abscess.

The discussion was opened by PROF. LANGALLI, of Pavia, who remarked that many diseases declared by physicians as scrofulous are really tubercular, such as osteitis, periostitis, etc. He believed that there is no difference in the lymph-ganglia of tubercle and scrofula, and that the lymphatic bodies in both assume the same changes, and that giant-cells and bacilli are found in both. He believed that scrofula is the prodrome of tubercle, and will end in it if not cured, and that it has its origin in disorders of the digestive organs.

PROF. CORNIL, of Paris, believed that the discovery of the bacillus settles the question of identity between scrofula and tubercle. Dr. Grancher's failure in two cases to prove immunity of the scrofulous discharge is probably due to the fact that the abscesses from which the virus was taken were of tubercular character. He believed that what mostly ought to be kept in mind here is that many diseases which formerly were classified with scrofula are now known to belong to tuberculosis.

DR. GRANCHER mentioned that probably the only difference between the two diseases is that one is caused by an "exalted" virus, while the other is the result of an "attenuated" virus, a view which is entirely supported by the researches of Prof. Pasteur; but he believed that this Congress will not bring this question to a conclusion.

PROF. WEIGERT, of Leipzig, did not believe in this theory of attenuation. He thought that the disease is localized in the lymphatic glands. In children, changes take place in these glands without being of tubercular nature; with adults, tubercular disease occurs without

implication of the glands. The former, he claims, is due to the capsule of the gland, which prevents the dissemination of the disease, while in adults the bacilli are absorbed through the mucous tracts. In adults tuberculosis appears first in the apices of the lungs, in children in the hilus, the latter caused by the eventual perforation of the capsules of the lymphatic glands around the hilus, and the favorable prognosis in children must mainly be based on localization, and not on attenuation.

DR. GRANCHER thought this view reasonable, but found the following objection to attributing the difference in the two diseases to age: 1. Scrofula is not confined to the child. 2. Adults have diseases of bony tissues, which usually pertain to childhood. 3. Children are not free from tuberculosis. Besides, the histological differences are very pronounced. He did not mean to say that vaccination of attenuated virus would protect against tuberculosis.

PROF. WEIGERT desired to state that he did not mean to convey the idea that only children have localization, but that most frequently such is the case.

During the general session in the afternoon PROF. VERNEUIL, of Paris, read a paper on the *Neoplastic Diathesis*. He attempted to prove by ingenious arguments, based on clinical observations, that all neoplasms which are not the result of a specific disease, are the result of a diathesis, which, according to the peculiarity of the individual, will in one case produce sarcoma, in another lipoma, in another cysts, etc.; that the number of neoplasms has nothing to do with the diathesis, and that the diathesis may produce neoplasms of different character in the same person, the whole of them partaking of the nature of a hyperplasia of the normal tissues; that their cause is pathological, the growth physiological, the immediate existing cause being either irritation, or of an idiopathic character.

PROF. CORNIL, of Paris, then read in the Section a paper on *Chronic Nephritis*, especially on the relation of the changes of connective tissue, parenchyma, blood-vessels, and heart in this disease. His views can best be condensed by giving his classification of the different kinds of albuminous nephritis:

1. Diffuse nephritis with—
 - a. Predominance of congestive and inflammatory phenomena.
 - b. Predominance of diapedic lesions.
 - c. Predominance of degenerative lesions.
2. Diffuse subacute and chronic nephritis with—
 - a. Predominance of lesions of glomeruli.
 - b. Predominance of lesions of epithelium.
 - c. Predominance of lesions of interstitial tissue.
3. Interstitial nephritis or necrosis.
 - a. Cirrhosis of glandular origin.
 - b. Cirrhosis of vascular origin (interstitial necrosis proper).

Prof. Cornil presented original researches on interstitial nephritis artificially produced in animals by lead poisoning. The kidneys show on transverse section of the pyramids in the centre, a fibrous zone with atrophic tubes, in the periphery a zone formed by the glomeruli surrounded by interstitial tissue and an intermediate fibrous zone with urinary tubuli, part of them atrophied, others almost sound, constituting the granulations prominent on the surface of the kidney. The elimination of the lead salts and the resulting deposits of calcareous salts are the cause of the chronic inflammation which terminates in atrophy of the epithelial cells.

In the commencement the bloodvessels are normal, and their walls become thickened only after the renal sclerosis is already well advanced. Albuminuria is not present in the guinea-pig, in the human being it may or may not be present; but in the chronic saturnine nephritis of the latter, coexisting with albumen, he found cellular lesions analogous to diffuse nephritis. Sometimes cysts form at the expense of the dilated tubes. The vessels which pass through the sclerotic tissue are in themselves sclerotic, but the larger arteries are not affected. Coexisting, he often noted hypertrophy of the left side of the heart, and deposits of sodium urate in the articular cartilage, especially in the phalango-metatarsal joint of the great toe, resembling those of gout.

SIR WILLIAM GULL presented a paper on the same subject. After introductory remarks on the fallacy of Bright's views that renal lesions are always of one form and character, have local origin, and are the source of the systemic lesions which are associated with them, with a vague idea that albuminuria indicates one pathology, the speaker attributed to an arterio-capillary fibrosis, which may begin in any part of the vascular area, the cause of nephritis. This, even with cardiac hypertrophy, may precede defect of renal functions. In this cachexia the intima and adventitia of the arterioles and capillaries are changed by fibroid or hyalin-fibroid deposit. The muscular coat is thickened. Death may occur from cerebral hemorrhage with cardiac hypertrophy, whilst the kidneys are found but little affected. The fibroid change in the kidney has no acute, inflammatory beginning, and its early stage is coexistent with vascular changes in other organs. The same condition of the vessels is found in other organs, as the lungs, stomach, skin, with or without renal disease, proving that the renal change has common characters with those more widespread through the organism. The hypertrophy of the heart is to be attributed to loss of arterio-capillary elasticity and hinderance to interstitial circulation of plasma; whilst in the cases in question the arterio-capillary changes cannot be referred to the kidney as their source, it still remains to be determined how far a change, beginning in the kidneys, may promote a cachexia inducing arterio-capillary fibrosis.

PROF. HELLER, in about one hundred cases, had not found the fibrous thickening.

PROF. PRÉVÔT found incrustations with calcareous salts (calc. carb.) in the kidneys in consequence of mercurial poisoning, with lessening of urinary secretion. In saturnine poisoning he found the same in the form of phosphates of lime.

PROF. CORNIL remarked that Sir W. Gull had left the domain of special pathology to discuss the subject from the point of view of general pathology. Special pathological researches cannot be formulated in that manner. Sclerosis commences in the walls of the tubes, and from there proceeds to the interstitial tissue and is of an epithelial origin. He cannot consider saturnine nephritis as a result of arterio-capillary sclerosis, as it is characterized at its outset by infarctions caused by elimination of lead salts and also by cell lesions; then come atrophy, collapse of the tubes, and the condensation of the connective tissue. Only long after this we observe the chronic arteritis. Considering that a rational classification of lesions has to be based on the origin and

development of disease, Prof. Cornil adheres to his view of two varieties of renal cirrhosis—that of glandular origin (saturnine and gouty nephritis), and that of vascular origin.

PROF. MAHOMED, of England, believed the change to be due to contraction of the arteries and consequent slowing of the movements of the blood corpuscles. This may be transient, but also chronic, as in gout, dyspepsia, saturnine poisoning. The first factor is altered blood, then come the changes in the kidneys, and, last, those in the vascular system.

The next paper was that of PROF. CHAUVEAU, of Lyons, on the *Possibility of Animal Tuberculosis becoming a Source of Human Tuberculosis*. Nothing is easier than to communicate tuberculosis to the animal; the guinea-pig and rabbit are especially susceptible; only the horse and donkey are refractory. Few species, however, produce idiopathically tuberculosis—only the pig and the beef. In the former it is exceedingly rare; in the beef, however, frequent—about one and a half per cent.—consequently this latter animal alone may be considered as possibly dangerous in that respect. He introduced tubercular infection from the beef into calves by way of the stomach (mixed with food), and they all became tubercular; any other pus was inert. It is therefore proved that the disease can be propagated from beef to beef. To prove the unity of tubercular matter in beef and human species, disputed by Virchow, calves were fed from human tubercular pus, and infection was the result. There is consequently a great danger of infection from the beef. This may be communicated in three ways: by the eating of beef, the use of milk, and animal vaccination. Statistics on the first are not complete, but lean rather to the negative; on the second we will hear a paper from Prof. Bang, with which he coincides; he will here principally speak of the third.

The first reason for bovine vaccination was to avoid the danger of syphilis. This practice has a great advantage on that account. He has made experiments as to the possibility of inoculating tuberculosis in using bovine virus. He injected vaccine matter from a tubercular cow into pigs and rabbits, and they all died from tuberculosis. Experiments on guinea-pigs vaccinated with vaccine virus from tubercular persons had negative results. Tuberculosis is very rare in young calves. He found it very difficult to produce it in animals by superficial scarifications as practised in ordinary vaccination. In five cases vaccination on erosions or by subcutaneous injection was negative as far as producing tuberculosis in calves; the same results were obtained in the guinea-pig. His conclusion was that if there is a possible danger of introducing tuberculosis by bovine virus, it is almost entirely neutralized by the nearly total impossibility of communicating it through the usual process of vaccination.

PROF. VALLIN, of Paris, found that *localized* tuberculosis in the beef does not produce tuberculosis in the guinea-pig on injection of the macerated juice of the muscular tissue. He stated that according to Prof. Chauveau's percentage of tuberculosis in beef, there were consumed, in Paris, last year over ten thousand tubercular beeves, slaughtered there (not counting the imported meat), and that only eleven carcasses were rejected by the inspectors for that cause.

PROF. SALOMONSEN caused tuberculosis in the guinea-

pig by injecting tubercular matter into the anterior chamber of the eye, but *blood* from tubercular animals, injected in the same manner, gave negative results. Injections into the peritoneal cavity of a guinea-pig with tubercular blood from another were likewise negative.

PROF. CHAUVEAU had made many experiments on the virulence of the *blood* in tuberculosis, and found that it is not virulent. He has had the same experience in glanders, where injection of the lymph reproduced the disease, but blood did not. The same in variola of sheep. His experiments on the juice of meat are not completed and are not so conclusive. Meat juice from tubercular beef, injected into a large number of guinea-pigs gave negative results, while matter from tubercular lymphatic glands caused tuberculosis in every case. Cooking for one hour at 70° R. did not destroy its virulence; at 100°, with one-half hour's cooking, however, it was lost.

PROF. CORNIL thought that the same obtains with the vaccine matter as with the blood, and that the question of possibility of tubercular infection by vaccine lymph, must be decided in the negative.

The next paper was by PROF. BANG, of Copenhagen, on *Tuberculosis of the Udder of the Cow*. This is not rare. He has seen twenty-seven cases; seven during the last seven months. The diagnosis is easy. A diffuse, painless, gradually hardening tumefaction of usually one-quarter of the udder takes place, without impairment of health or decrease in the quantity or appearance of the milk (this is important because this milk already holds tubercular bacilli, and is still used as food. When bacilli are associated with spores, according to Koch, they resist the action of the gastric juice). Gradually, usually not before a month, the secretion of the gland assumes a watery, flocculent appearance, but is not purulent. The animals generally perish inside of three months. Injection of the milk produced death from tuberculosis in rabbits in from two to three months. The later serous secretion caused fatal results from tubercular peritonitis in twenty days. Experiments of feeding rabbits on this milk have not yet been concluded. He found that milk from a tuberculous cow, without localization in the udder, was virulent. Heating to 212° F. destroyed the vitality of the bacilli.

PROF. HELLER, of Kiel, then made some remarks on *Congenital Syphilis of the Lungs*, illustrated by sections under the microscope. The main anatomical change found is a capillary hyperplasia in the interstitial tissue, with hypertrophy of the right ventricle. The children usually die before the end of the first year.

DR. NELSON, of New York, presented a paper on the *Culture of Diphtheritic Microbes*. He assisted last November in tracheotomy on a diphtheritic child. A membranous cast was removed. The child died thirty-six hours later. He inoculated one of Sternberg's culture bulbs with some of the membrane. (For culture, the bulbs should be kept at 100° F. for several weeks.) In four days the sterile liquid became cloudy, and with 1000 diameters, immense quantities of micrococci similar to those of pus were found, which he propagated successfully through ten generations. This propagation usually took ten days. He inoculated three guinea-pigs in the anterior chamber; one died in thirty-six hours. Two were killed in three days. In all the eyeballs were

destroyed, and micrococci were discovered in large numbers. He believes that there is a micrococcus of diphtheria which will reproduce the disease in guinea-pigs.

The last discourse was by PROF. VIRCHOW, on *Forms and Causes of Hepatitis*. While in nephritis the arteries play the principal rôle, in hepatitis the veins are the affected part. It is difficult or impossible to follow the changes proceeding from the capsule of Glisson to the nervous and arterial system, but he believes that a chronic fibrosis affects the veins and bilious ducts. This fibrosis is unequal; the liver has a tubercular surface, the depressions being the result, the tubercles the original line. This is particularly striking in syphilitic forms. For localization of the latter some natural factor may be the determining cause; as we find a gummatous tumor originating from a contusion of the forehead, so in the liver the minute superficial ruptures caused in concussions, especially along the ligaments, may be the exciting cause, and these changes are also mainly found along these ligaments; other causes for these localizations are so far not known. Those who reduce all affections in anticipation to microbes, may also find an explanation, without actually demonstrating the microbes. In opposition to this view, he gave examples in which the predisposing cause is a chemical poison. Thus minimal doses of phosphorus will produce a cirrhotic condition of the liver (experiments on chickens). But even here the actual cause is unknown. The cirrhosis is uniform in acute phosphorus poisoning, while in the chronic form its distribution is irregular. We may find a parallel in the malarial liver. In common with syphilis, it appears in different forms, but has not the large foci of the syphilitic liver, and might best be likened to a general induration, and it may become granular with irregular distribution. It has also frequently a melanæmic appearance, leading to possible confusion with putrefaction. Frerichs believed this to be a result of embolism, but Virchow has examined many cases, and found only the usual contents of the vessel, but there was a melasma with black connective tissue due to pigment in the cells. This may be produced by other causes than malaria, but he knows of none. Here, also, the reason why it is unequally distributed is not known. He also referred to alcohol as a cause for fibrosis. We have, therefore, four different causes for bringing about parallel changes, viz., syphilis, phosphorus, malaria, and alcohol.

One more point is important. Cirrhosis frequently occurs with chronic peritonitis, often also with the appearance of tubercles, which, however, have no tendency to produce the cirrhotic change, and differ from the appearances in general tuberculosis. These simple forms are complicated in many ways in nature.

As to predisposition, it may be found in the peculiarities of the anatomy differing in every individual in the normal state, especially as to the size, and, consequently, the number of the acini and their supply of capillaries (different arrangement and quantity of vessels). This will explain the fact that the diseases originating from the same cause show various effects in different individuals. We must always consider the cause by itself, and the local result by itself.

In reply to a remark of Dr. Fränkel, referring to hyperplastic changes, Virchow said that he had seen both in the same case, resembling externally carcinoma.

PROF. HELLER mentioned that he had also seen the so-called hypertrophic form without apparent tendency to shrink, with no malaria for a cause. Another possible cause which has not been mentioned is strong spices, which, he believes, may irritate the liver.

This last discourse of Prof. Virchow having been made without notes, was, as usual under such circumstances, at times disconnected, and it was difficult, often, to follow the main thread.

THE SECTION OF DERMATOLOGY AND SYPHILIS.

(From another Correspondent.)

COPENHAGEN, August 17, 1884.

THE Section of Dermatology and Syphilis opened at 9.30 A.M., with THE PRESIDENT, DR. HASLUND, of Copenhagen, in the Chair.

PROF. DOUTRELEPONT, of Bonn, read a paper on *Lupus Vulgaris*, holding it to be tubercular in origin and nature. The discussion was participated in by Leloir, Kaposi, Neisser, Unna, and others.

PROF. KAPOSI held that we must distinguish between lupus vulgaris and tuberculosis of the skin; that they are two separate diseases. Of the latter he had seen thirteen well-marked cases, ten in the clinic, and three in private practice. Prof. Pick agreed with Kaposi. Leloir thought, with Koch, that there was a bacillus in lupus; and Neisser, Doutrelepon, Unna, and Lipp concurred in this view.

On August 12th, PROF. KAPOSI occupied the Chair. The first paper was by Dr. Armauer Hansen, of Bergen, on *Norwegian Leprosy, its Etiology and Pathology*. He presented three marked cases which he had sent to the meeting for exhibition, from Norway. One was especially interesting, as showing the results of the disease, having the phalanges of both hands and feet greatly distorted, with even a loss of several. The muscles of the face were so atrophied as to make it impossible to bring them into action, and the eyes remained widely separated even during sleep. He observed a vast difference between the knotty and the smooth variety. The course is regular until anæsthesia sets in, when he could stick a knife into the patient without the least pain. Even limbs could be amputated without pain. Patients with the knotty variety may die of the disease, the usual course being to succumb to the sequelæ. There are various clinical phenomena which speak for the contagious character of this disease. "Looking toward the etiology, it must be remarked that inheritance and contagion bar each other; so that we have no contagious disease which is at the same time inherited." The bacillus lepræ has not yet solved the question, but has shown as quite probable the contagious nature of the disease. Isolation is the best preventive measure, as was shown by a series of statistics.

PROF. ZAMBACO, of Constantinople, presented a number of plates illustrative of leprosy.

PROF. PICK, of Prague, read a very exhaustive paper on *Excision of the Initial Sclerosis of Syphilis*. He cited many cases to substantiate his views, and came to the conclusion that excision should be done even in doubtful cases, so as to give the patient the benefit of the doubt. Besides, a clean cut heals by first intention, whereas the sore, if left, or otherwise destroyed, will remain indefinitely.

DR. PONTOPPIDAN demonstrated the views of Prof. Pick in several cases, and coincided in the opinions he expressed.

PROF. MARTINEAU, of Paris, said we should be very careful to diagnosticate between induration and sclerosis, and not cut every case.

The Section adjourned to meet on the 14th, when PROF. PICK occupied the Chair.

DR. GRUNFELD, of Vienna, exhibited an *Endoscopic Case* of his own design, with various sized tubes and a mirror such as is used by laryngologists. He showed, by drawings, how different portions of the urethra presented different pictures, and each very characteristic of its situation.

PROF. LIEBREICH, of Berlin, read a paper on the *Treatment of Syphilis by the Injection of Mercury*, and made many interesting chemical tests to prove which salts of mercury were the best to use. He thinks that besides corrosive sublimate, formic acid is the best. When mercury ceases to act, the original susceptibility can be restored by a good diet with an abundance of salt, when more of the mercury will be again absorbed.

DR. WULFF, of Strasburg, said it should be our object to select a remedy that would be as nearly painless as possible, and that we should recollect that besides the disease we still have a patient to deal with. Profs. Martineau, Bergh, Lipp, Neisser, Kaposi, and others, did not favor the injections as being better than other methods.

PROF. KAPOSI said the injections appear at first to be the best, but after a while they lose their effect, whereas inunctions do not give at first such brilliant results, but by-and-by they prove more satisfactory, and in the end achieve better results. After considerable discussion it seemed to be the prevailing opinion that no one method was yet settled as *par excellence* the best.

On Friday, August 15th, PROF. DOUTRELEPONT occupied the Chair.

PROF. LANG, of Innsbruck, presented a paper on the *Combination of Carcinoma and Gummata*. He showed a series of photographs and diagrams to illustrate his views, and one case, he said, was undoubtedly marked. He had been unable to find any history of cancer, or the existence of it before the appearance of the gummata, and was of the opinion that it came on after the latter had healed.

DR. BARTHÉLEMY, of Paris, then read a very interesting communication on the *Visceral Manifestations of Late Hereditary Syphilis*.

PROF. NEISSER then spoke of the *Period of Cessation of the Contagion of Gonorrhœa*. He said it was difficult to settle such a question unless we know absolutely the last act of coitus, and as long as the bacillus exists it is contagious, and he would not advise marriage. He thought probably three months long enough.

On Saturday August 16th, PROF. THIN, of London, occupied the Chair. As this was closing day, he requested that the debates be limited so as to allow all papers to be read that were on the programme.

PROF. JANOWISKY, of Prague, read a paper on *Iodoform-exanthem*, and he reported that he had seen many cases the result of the absorption of the drug.

PROF. HASLUND had used iodoform very extensively, but had only seen one marked case, which occurred on the third day after the extirpation of a bubo, and the

wound was packed with iodoform. There was no fever, at first vesicles appeared, then pustules, and in a few days these dried up.

DR. HASLUND, of Copenhagen, read the closing paper on the *Pathogenesis of Gonorrhœal Rheumatism*.

PROF. DOUTRELEPONT, in the Chair, then thanked the President, Dr. Haslund, and the secretaries for their services, and again wished to renew his thanks and that of the members to the committee for their successful management of the Congress. The Section then adjourned.

THE BERLIN LABORATORIES.

BERLIN, August 18, 1884.

A SHORT account of the Berlin laboratories, the work of which has a direct bearing upon medicine, may possibly prove interesting to the readers of THE MEDICAL NEWS.

And first of all, of chemistry, to which directly and indirectly medicine owes so much and to which in the future, possibly the near future, it is likely to owe much more. There are several good chemical laboratories here in which a "mediciner" may work. That of Prof. Hofmann, on account of the fame of its "director," is filled and more than filled. The regular lectures on chemistry are given three times weekly, in sessions of two hours each. They are most admirably illustrated by experiment, and the subjects dealt with rendered very clear, in fact, some think there is an excess of simplification, but this is a good fault, if fault it be.

It is well for any foreigner, who may wish to obtain a place in this laboratory, to know that it will be necessary to apply long before the expected period of work, and even then he may find, as I did, that a place in "Hofmann's Laboratory" does not mean necessarily a place under Prof. Hofmann himself. I was, in fact, consigned to another department of the laboratory which the distinguished professor himself never even visits so far as I know, and which is presided over by another professor. As my own experience of this latter department of the laboratory does not enable me to speak favorably of it, and as such experience may possibly be exceptional, and I sincerely hope it is, I pass this laboratory by without further remark.

I have heard Prof. Liebermann and his laboratory well spoken of, but know nothing of them from personal experience.

Of laboratories for physiological chemistry there are several in Berlin, each of them under the direction of a teacher of distinction. The oldest of these is that in the Pathological Institute in connection with the Charité. It was founded as far back as 1856, at a time when laboratories for physiological chemistry, even in Germany, were very insignificant as a rule. This laboratory is a monument of the insight of the great Virchow, to whom it owes its existence. Even at this early period Virchow could see the relation and importance of chemistry to medicine, which not a few of some standing in our profession seem not yet to realize. Such distinguished men as Hoppe-Seyler and Kühne, have had a connection with this laboratory in earlier days, and it is now presided over by Prof. E. Salkowski, who has recently become still better known since the publication, in conjunction with Leube, of his large and elaborate work on the urine. From personal experience of this

laboratory, I can recommend it to anyone wishing to work on medical chemistry, whether physiological or pathological. The Physiological Institute has also an admirable laboratory for physiological chemistry under the direction of Dr. Kossel, who was till recently an assistant of Prof. Hoppe-Seyler in Strasbourg.

It is remarkable how many "practical physicians" still, at the age of thirty-five to forty, manage to maintain a lively interest in physiology, and often undertake investigation either in the chemical or experimental department. Sometimes this work has a direct reference to practical medical questions, and sometimes seems to be done purely to maintain an old scientific love. They have never been, in fact, divorced.

The Institute for Physics under the direction of Helmholtz, that for Physiology under Du Bois Reymond, and that for Pharmacology under Liebreich, are so close together that they form one magnificent and compact pile, only surpassed in beauty by the Military Academy opposite. A course on physiological optics is being given in the Institute for Physics, but the "mediciner," who would get much from this, must be well versed in mathematics. The case is entirely different with another course on physics, viz., that given in the Institute for Physiology by Prof. Christiani. This course is especially designed for the "mediciner," and comprises what is most important for the understanding of experimental physiology, the microscope, etc. The sessions are of two hours' length, and occur twice weekly. The experiments are most admirable and leave little to be desired. The course is modelled somewhat after Tyndall's public lectures, but is still different in that theory is somewhat more prominent, and the mathematics of the subject are not neglected. However by the use of the *graphic method*, many mathematical formulas, which otherwise would be very difficult for such auditors, are rendered clear. The same remark applies to the more difficult theories, such as the different wave theories. Altogether the course is *unique*, and the teacher of any department of experimental science can get many valuable suggestions as to the best methods of making his subject clear and impressive.

Physiology is in a highly flourishing condition in Berlin. Reference has already been made to the abundant provision made for physiological chemistry. A course of lectures on general physiology is given at the excellent Veterinary School here by Prof. Munk. They are both clear and well adapted to the hearers. The principal course of lectures in the Physiological Institute is given by the venerable and long famous director, Prof. Du Bois Reymond. His lecture-room is large, comfortable, and exceedingly elegant in all its appointments, and the lectures are illustrated with the most beautiful and best adapted apparatus to be had.

Prof. Hugo Kronecker also gives one lecture a week on the physiology of the heart and one on muscle physiology, both in much detail and well illustrated.

In the experimental department of physiology, the laboratory work devolves mostly upon the energetic and accomplished professor just named.

Apart from conducting many special researches, a course on "methods of investigation" in physiology is given. In this course vivisection and the graphic method are taught very much apart, and in general the separation of what may be called the *technique* of phy-

siology from the theory, and the facts of the science observed in this laboratory seem to me worthy of much consideration. How much loss of time and confusion of ideas is avoided by such a course, only those who have seen the reverse method pursued can thoroughly appreciate. I should be glad to see such a separation in some way applied to chemistry and some other departments, for I know of no laboratories in which so much time is simply pottered away as in chemical laboratories. A preliminary course on *technique* or manipulation would obviate much of this and also probably prevent the student from becoming the merely mechanical worker. Prof. Kronecker's full course in "methods of investigation" extends over four semesters or two years. I must not omit to remark upon the exceedingly courteous, in fact cordial treatment, all students, including foreigners, receive in this professor's laboratory. Here, at least, the man and the gentleman are not forgotten in the student, nor lost in the professor.

From what has been said, it will appear that ample provision is made in the Berlin Physiological Institute for the teaching under the best conditions of the physics of medicine, the chemistry of medicine, and the experimental physiology of medicine; and it only remains to add that a full course in histology, etc., is given under Prof. Fritsch, to indicate how thoroughly this Institute deserves its name.

The adjoining pharmacological laboratory under the direction of the well-known Liebreich, is, in the completeness of its various departments, perhaps unequalled anywhere in the world. No expense seems to have been spared to make it as nearly perfect as possible for the purposes, both of teaching and of investigation. A noteworthy regulation exists requiring all students to take a course of instruction in antiseptics, and a special department of the laboratory is devoted to this purpose.

The Berlin Physiological Society has a large membership, and the attendance at the meetings, which are held every two weeks, is uniformly good. Prof. Du Bois Reymond occupies the presidential chair. Abundance of material in the form of papers is forthcoming, and each communication is subjected to the keenest criticism by those well able to discuss any physiological question that may arise.

Other laboratories having a relation to medicine might be referred to. In conclusion, I may be allowed to express the hope that before long the authorities of the Berlin University will see fit to abolish the regulation by which all foreigners, irrespective of age and *status*, are obliged to pay a matriculation fee and go through all the forms (and these are not a few) pertaining to matriculation, before they can participate in the advantages offered by this great University.

T. W. M.

NEWS ITEMS.

THE COLLEGE OF PHYSICIANS OF PHILADELPHIA on Monday evening gave a reception to the members of the British and American Associations for the Advancement of Science who were in attendance upon the Philadelphia meeting. The building was beautifully decorated with flowers and plants, and the Library and Museum were open. Refreshments were served in the Lecture Hall.

This was the first of the series of entertainments provided for by the Weir Mitchell Entertainment Fund.

DR. JOHN G. ADAMS'S MEDICAL BEQUESTS.—Dr. John G. Adams, of New York City, by his will dated June 21, 1881, has left \$5000 to the Presbyterian Hospital for a free bed for the sick poor of the Brick Church; \$5000 to the Academy of Medicine to pay off the debt on its building (this bequest is revoked by codicil on account of the action of the Academy in reference to the new code); \$2500 to the Bridgeport Protestant Orphan Asylum and Hospital; \$500 each to the Physicians' Mutual Aid Association, and the Training School for Nurses. His medical library is bequeathed to the New York Academy of Medicine.

THE CONSOLIDATION OF THE MEDICO-CHIRURGICAL COLLEGE WITH THE PHILADELPHIA DENTAL COLLEGE.—We understand that a consolidation of these two schools has been effected, and while each will for the present maintain its separate organization, the students of the former will receive instruction in surgery, physiology, and chemistry at the Dental School, while the dental students will receive clinical instruction in other subjects than dentistry at the former.

CHOLERA MORTALITY AT MARSEILLES.—Dr. Albenois, of Marseilles, in the monthly *Bulletin* of the Bureau of Statistics for July, gives some very interesting statistics relative to the epidemic of cholera in that city. From June 27th, the date on which the disease was recognized as true cholera, to August 4th, the deaths numbered 1311, of which 844 occurred in the city, 163 in the outskirts, and 304 in the hospitals. The period during which the disease seemed to have reached its maximum of fatality, was from July 11th to July 26th, in which period there were 853 deaths, a daily average of 51; from July 26th on there was a marked and steady decrease in the deaths. Of the decedents, 5 were under one year of age, 52 under five; 1029 between five and sixty, and 162 over sixty years of age.

Another interesting point is the mortality among those of different nationalities. Of the decedents, 853 were French, and 330 Italian, the latter paying large tribute to the epidemic, the proportion being 5.70 per 1000, while for the French it was 2.93, and for all other foreigners 1.36 per 1000. Dr. Albenois's explanation of this high mortality among the Italians is, that they are, as a class, extremely poor, and engaged in the hardest and most repugnant work, receiving but small wages, at the same time having large families, and living in the most unhealthy sections, in close and crowded quarters, often deprived even of the necessities of life, it is then not at all astonishing that they fall easy victims to the epidemic.

During the month there were attributed to diarrhoeal diseases 272 deaths. The total number of deaths was 2478; and the annual death-rate 82.6 per 1000.

PROF. CARL WEDL.—The Emperor of Austria has conferred the title of "Hofrath" upon Dr. Carl Wedl, Professor of Histology in the University of Vienna, in recognition of his long and useful services to science.

OBITUARY RECORD—JULIUS COHNHEIM.—The closing days of the International Medical Congress were saddened by the announcement by Prof. Virchow, in the

Section of Pathology, of the death from gouty nephritis, in Leipzig, on August 14th, of his distinguished pupil, PROF. JULIUS COHNHEIM. A telegram of condolence was immediately sent to his widow by the Section.

Prof. Cohnheim was born in Demmin, in 1839, and studied medicine from 1856 to 1860 in Berlin, Würzburg, Griefswald, and Prague. He commenced to practise in Berlin, but was appointed, in 1864, assistant to Prof. Virchow in the Pathological Institute, when he entered upon a new life. In 1868 he was appointed Ordinary Professor of Pathological Anatomy in Kiel. He soon left Kiel, however, to succeed Waldeyer in Breslau. His work here was probably the most brilliant and fruitful of his whole life; so much so, that he was called to the Pathological Institute, in Leipzig, on the occurrence of the first vacancy. His name will chiefly be remembered for his contributions to the doctrine of inflammation. His discovery of the migration of the white blood-corpuscles through the walls of the capillaries in inflammation is his chief title to fame. It is too early, says the *Deutsche medicinische Wochenschrift*, to estimate the great influence of his work, and we can only know that in his death practical pathology has sustained an irreparable loss, even though in his "Vorlesungen" he has left a work, for him an enduring monument, which combines physiology and pathology to one extent never before attained. Few more important truths have been formulated in medicine than Cohnheim's, "without bloodvessels no inflammation is possible."

—On September 6th, in this city, aged seventy-seven years, ROBERT EMPIE ROGERS, Emeritus Professor of Chemistry in the Jefferson Medical College.

Dr. Rogers received his medical education at the University of Pennsylvania, and graduated in 1836. From 1844 to 1852 he held the Chair of Chemistry in the University of Virginia, and from 1852 to 1877 the same Chair in the University of Pennsylvania, and for the last seven years he has occupied the Chair of Chemistry in the Jefferson Medical College.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM SEPTEMBER 2 TO SEPTEMBER 9, 1884.

BYRNE, CHARES C., *Major and Surgeon*.—Relieved from duty in Department of California, and to report in person to the Commanding General, Department of the Platte, for assignment to duty.—S. O. 207, A. G. O., September 3, 1884.

TOWN, F. L., *Major and Surgeon*.—Relieved from duty in Department of the Columbia, and to report in person to Commanding General, Department of Texas, for assignment to duty.—S. O. 207, C. S., A. G. O. Granted leave of absence for twenty-five days.—S. O. 127, Department of the Columbia, August 25, 1884.

HAVARD, VALERY, *Captain and Assistant Surgeon*.—Relieved from duty in the Department of Texas, and to report in person to the Commanding General, Department of the East, for assignment to duty.—S. O. 207, C. S., A. G. O.

HALL, WM. R., *Captain and Assistant Surgeon*.—Relieved from duty in Department of Texas, and to report in person, on October 1, 1884, to the Superintendent-General of Recruiting Service, in New York City, for duty at David's Island, N. Y., relieving Assistant Surgeon M. E. Taylor from duty at that station.—S. O. 207, C. S., A. G. O.

HOPKINS, WM. E., *First Lieutenant and Assistant Surgeon*.—The leave of absence granted him in S. O. 67, August 7, 1884, Department of Arizona, is extended one month.—S. O. 204, A. G. O., August 30, 1884.